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of

THE ROYAL AERONAUTICAL SOCIETY

AUGUST 1961

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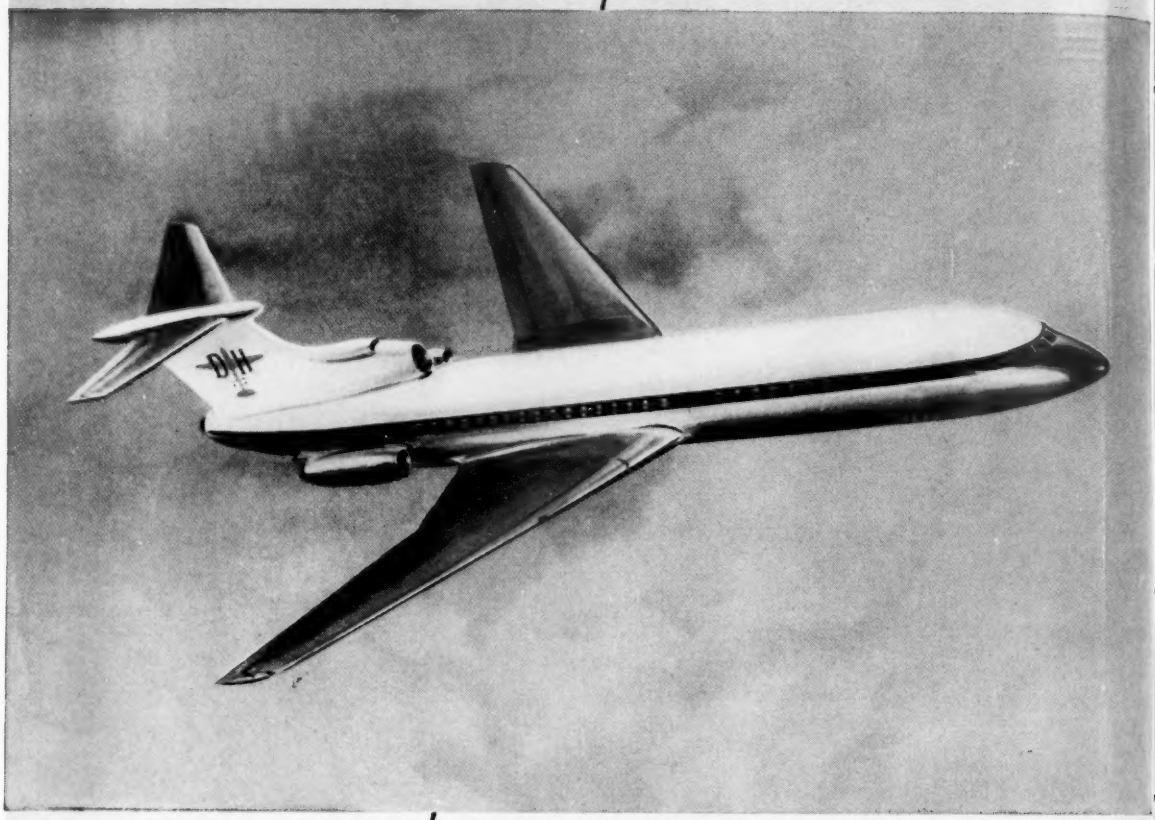

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INCORPORATING THE INSTITUTION OF AERONAUTICAL ENGINEERS AND THE HELICOPTER ASSOCIATION OF GREAT BRITAIN

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All communications for publication in the JOURNAL *should be addressed to the Editor,* JOAN BRADBROOKE, *A.R.Ae.S.; all communications on general matters affecting the Society should be addressed to the Secretary,* A. M. BALLANTYNE, *T.D., B.Sc., Ph.D., F.R.Ae.S., Hon.F.C.A.I., F.I.A.S.*

None of the papers or paragraphs must be taken as expressing the opinion of the Council of the Royal Aeronautical Society unless otherwise stated.

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NOTICES

Scholarship Selection Board

The Edward Busk Studentship in Aeronautics has been awarded to Brian Herbert Jones of 2 Endsleigh Gardens, Upton Heath, Nr. Chester. Mr. Jones is 24 years old, and has just obtained the degree of B.Eng. with First Class Honours at the University of Liverpool. He is to remain at Liverpool to do research on elastic plastic deformation and fracture of thin-walled tubes under different bi-axial stress states. Mr. Jones served an engineering apprenticeship at de Havilland Aircraft Co. Ltd., and in 1957 was judged the best apprentice in all the de Havilland companies in this country.

The Geoffrey de Havilland Memorial Scholarship has been awarded to Douglas William Laurie-Lean. Mr. Laurie-Lean is 25 years old and is an Australian. He obtained the Diploma of Aeronautical Engineering at the University of New South Wales while he was a Cadet Apprentice at the de Havilland Aircraft Pty. Ltd., at Bankstown, Sydney, and after two years with the company there he joined the company at Hatfield in 1960. He holds a private pilot's licence. Mr. Laurie-Lean is now going to take the two-year Diploma Course at the College of Aeronautics, Cranfield, specialising in Aerodynamics.

The Charter Scholarship has not been awarded for 1961-62.

Royal Aeronautical Society—University Prizes

The following have been awarded Royal Aeronautical Society University Prizes for 1961:—

Cambridge University: S. A. M. Thornley, St. John's College.
Bristol University: B. W. Lowrie.
Southampton University: R. L. Butchart.
Imperial College, London: P. C. Rumsey.
Glasgow University: D. A. Cameron.
College of Aeronautics: Flt. Lt. C. B. Stribling.
Queen Mary College, London: T. R. Crossley.

Elliott Memorial Prize

The Elliott Memorial Prize for the Second Term in 1961 has been awarded to Leading Apprentice T. Heap, and will be presented at the Halton Graduation Prize-giving.

Henlow Cadet Prize, 1961

Technical Cadet D. K. Tucker has been awarded the Henlow Cadet Prize for 1961.

Associate Fellowship Examinations—December

The closing date for Candidates in the United Kingdom for December 1961 Examinations is 31st August. Entry forms may be obtained from the Secretary. The closing date for entries outside the U.K. was 30th June.

Acknowledgments

The Council wishes to thank H. F. Cowley (*Companion*) for presenting a relic of historical interest. It is a telegram handed in at Bexley on 1st August 1894 and addressed to Sidney Hollonds, care Siemens Bros., Telegraph Works, Woolwich. It reads "Machine lifted ten thousand pounds but is off track come and see it today—Maxim."

The Council thanks Trans-Canada Air Lines for the gift of nine Commemorative Airmail Covers including covers flown on the Inaugural Flights of the Viscount, Vanguard and DC-8 with the Company; ELAL Israel Airlines Ltd., for the gift of nineteen Commemorative Airmail Covers, five flown each way on the inauguration of their Boeing 707 Tel-Aviv-Paris-New York service; and Alitalia Italian Airlines for the gift of nine Commemorative Airmail covers flown on the inauguration of the Caravelle and DC-8 on their services.

Wilbur Wright Lecture—and Reception

The Forty-ninth Wilbur Wright Memorial Lecture will be given during the Eighth Anglo-American Aeronautical Conference by Dr. Abe Silverstein, F.R.Ae.S., F.I.A.S., Director of Space Programs, N.A.S.A., on Thursday 12th September at **7 p.m.** in the Lecture Theatre, 4 Hamilton Place.

The lecture will be followed by a Reception and Buffet Supper at 4 Hamilton Place, which will be open to **all** members of the Society and their ladies, whether or not they are attending the Conference. Tickets for the Reception, price £1 5s. 0d. each, should be applied for **by 23rd August.**

Fellowship

The following have been elected to Fellowship:—

John Elliston Allen, Head of Aerodynamics, Projects and Assessment Dept., Weapons Research Division, A. V. Roe & Co. Ltd.

Joseph Black, Head of Dept. of Aeronautical Engineering, Bristol College of Science & Technology.

Robert James Clayton, Deputy Director, Hirst Research Centre, and Manager, G.E.C. Applied Electronics Laboratories.

Archie T. Colwell, Retired Vice-President of Engineering, Director and Consultant, Thompson Ramo Wooldridge Inc., Ohio.

Arthur Barry Haines, Chief Aerodynamicist, Aircraft Research Association Ltd., Bedford.

Frederick William Walton Morley, Chief Design Engineer, Rolls-Royce Ltd., Derby.

Sydney Vincent Sippe, Retired.

William Henry Wittrick, Lawrence Hargrave Professor of Aeronautical Engineering, University of Sydney.

Crack Propagation Symposium—Cranfield

Members wishing to take part in the Symposium on Crack Propagation which is to be held at the College of Aeronautics, Cranfield, 26th - 28th September, should apply immediately to the Society for registration forms. Accommodation is limited and only the first applicants can be considered. The Registration Fee is £2 10s. 0d., and the inclusive charge for accommodation and meals from dinner on Monday 25th September to breakfast on Friday 29th September is £8 0s. 0d.

Factory Equipment Exhibition—Tickets

The organisers of the 1961 International Factory Equipment Exhibition, which is to be held at Earls Court, London, from 13th - 18th November, have kindly offered complimentary admission tickets to members of the Society. The Exhibition, which is sponsored by the London Chamber of Commerce, *Industrial Equipment News* and *The Financial Times*, will be grouped this year in 14 separate sections, each one corresponding to a major division of industry. Several new industries will be represented for the first time. Conferences, under the auspices of the British Productivity Council and the Automatic Vending Machine Association, will be held during the Exhibition.

Members wishing to attend the Exhibition should apply for tickets to Mr. A. A. Smith, International Factory Exhibition, Industrial and Trade Fairs Ltd., Commonwealth House, New Oxford Street, London, W.C.1 (Telephone: Chancery 9011).

American Astronautical Society—Eighth Annual National Meeting, 16th-18th January 1962

A three-day national meeting on the astronautical sciences will be held in the Sheraton-Park Hotel, Washington, D.C., from 16th-18th January 1962. The technical sessions will cover lunar and planetary exploration, guidance and control, communications, astrodynamics, applications of astronautical systems, astrobiology (bio-astronautics), system design and basic research.

Elections

The following is a list of elections and transfers of membership of the Society:—

Associate Fellows

Nigel Robert Allen
John Michael Bennett (*from Graduate*)
Michael Jeremy Bennison (*from Graduate*)
Robert Charles
Albert Edward Clifford (*from Associate*)
Donovan John Flood
Patrick William Joseph Fullam (*from Graduate*)
John Raymond Edward Fullerton
Raymond Ronald Rex Hancock
Ernest Frederick Hulse (*from Associate*)
Tjerand Lunde
William Norman Clyde Moore (*from Graduate*)
Kenneth Gerald Frank Noble (*from Graduate*)
Laurence Charles Neill Penney (*from Associate*)
Kenneth Petrie
Maxwell Smith
Brian Stevens (*from Graduate*)
William John Sutcliffe
Edmund Joseph Talbot Weiss (*ex Student*)
Clifford George Wing (*from Associate*)

Associates

Patrick Joseph Foley
Kenneth Francis John Jarvis

Graduates

Brian Cheal
Anthony McCabe (*from Student*)
John Brian Milburn (*from Student*)
William Keith Osborne (*from Student*)
Paul Francis Read (*from Student*)
Anthony Hugh Whitmore (*from Student*)

Students

Choon Chandrsuda
James Dalgarno McCredie
Anthony Thorne

Companions

Anthony Richard Barrowclough
Ip Ching
John George Slade
Ian Hilary Steels (*from Graduate*)
Roger Charles Taylor

Supplement to the Oxford English Dictionary

The following is the fourteenth list of aeronautical terms for the Supplement to the Oxford English Dictionary for which assistance in tracing earlier references is required. If members know of an earlier use than that given for any word in the following list they are asked to write to the Editor, Oxford English Dictionary Supplement, 40 Walton Crescent, Oxford, gving the reference(s), date, author, title, chapter and page.

heliport (helicopter station)	1948	*American Aviation*, 1st June
hump resistance	1931	*Flight*, 16th January
hump speed	1923	*Proceedings of the Third Air Conference, London*
hydrovane (on flying boats)	1933	*Aeronautical Terms* (*B.S.I.*)
iced up, icing up (of aircraft)	1929	R. Duncan, *Air Navigation and Meteorology*
intake (of jet engines)	1944	*War Illustrated*, 10th Nov.
interceptor (aircraft)	1930	*Flight*, 27th June
intercom. (esp. in aircraft)	1940	A. A. Michie and W. Graebner, *Their Finest Hour*
intruder (of aircraft)	1941	*The Aeroplane Spotter*, 9th October
jet (ellipt. = jet aeroplane)	1944	*Collier's*, 22nd April
jet aeroplane, aircraft, etc.	1944	G. Smith, *Gas Turbines and Jet Propulsion*
jet (air-)liner	1948	*News Chronicle*, 7th April
jet engine	1943	*Journal of the Royal Aeronautical Society*, XLVII
jet fighter	1944	*Saturday Evening Post*, 6th May
jet-propelled	1936	*Daily Sketch*, 17th November
jet propulsion	1867	*Annual Report of the Aeronautical Society of Great Britain*
jet tunnel	1934	*Aircraft Engineering*, June
jet turbine	1945	*Scientific American*, June
jink (manoeuvre aircraft)	1940	*The Aeroplane*, 30th August

News of Members

A. J. Barrett (*Associate Fellow*) Head of the Technical Department, Royal Aeronautical Society, has been awarded the degree of Ph.D. of London University.

B. D. Blackwell (*Associate Fellow*) Deputy Chief Engineer, Aero Research and Development, Bristol Siddeley, has been appointed Business Manager, Aero.

H. S. Bluston (*Student*) formerly Senior Aerodynamics Engineer, Republic Aviation Co., Long Island, U.S.A., is now Senior Scientist with T.R.G., New York.

Sqn. Ldr. L. Byram (*Associate Fellow*) having completed the course at the R.A.F. Staff College, Andover, is now on the Staff of the Weapons Systems Engineering Wing at the R.A.F. College, Henlow.

A. L. Cattermull (*Associate Fellow*), formerly A.I.D. Central Headquarters, Chessington, is now P.I.(A.), North Midland A.I.D. Region. (Correction to the July Journal.)

Cdr. J. T. Checketts, R.N. (*Associate Fellow*) formerly with the British Embassy, Rio de Janeiro, is now Assistant Naval, Military and Air Attache, Brazil.

L. C. Collins (*Associate Fellow*) formerly with Kaman Aircraft, is now Senior Engineer, Airframe Section, Vertol Division of Boeing Airplane Co., Morton, Pennsylvania.

Lieut. A. C. Cowin (*Graduate*) having completed the Guided Weapons Course at Shrivenham, is now with the Guided Weapons Section of the R.A.E.

R. W. Cross (*Associate Fellow*) formerly Ground Service Engineer, B.P. Kuwait, is now with B.P. (Eastern Agencies) Ltd., Bahrain.

Sqn. Ldr. G. T. Foden (*Associate*) formerly O.C. Ground Engineering Squadron, R.A.F., Bassingbourn, has retired from the R.A.F. and joined Kellogg International Corporation, Oil Refinery Engineers, in the Design Engineering Department.

Sqn. Ldr. G. B. Foort (*Associate*) formerly S.T.O. at R.A.F. Acklington, is now Technical Co-Ordinator—2 H.Q. Far East Air Force, Changi.

F. R. Green (*Associate Fellow*) formerly Chief Technician, is now General Manager, Fairey Aviation Co. of Australia Pty. Ltd.

J. A. Jefferson-Loveday (*Associate Fellow*) formerly with Rolls-Royce Ltd., is now with Hawker Siddeley Brush Turbines at Hucclecote.

B. P. Laight (*Fellow*) Director and Chief Engineer, Blackburn Aircraft Ltd., has been appointed a Director of Blackburn Electronics Ltd.

J. D. Mann (*Associate Fellow*) formerly Technical Officer, Central African Airways Corporation, is now Quality Control Engineer with the Ford Motor Co. of South Africa, Port Elizabeth.

C. H. Miller (*Associate Fellow*) formerly at R.A.E. Henlow, is now Head of the Department of Advanced Technology, at the Technical College, Farnborough.

Sqn. Ldr. F. J. Moden (*Associate*) formerly in the Directorate of Aircraft Engineering, Air Ministry, is now Senior Technical Officer, R.A.F. Element, R.A.A.F. Base, Butterworth, Penang.

Commander B. B. Mungo (*Associate Fellow*) formerly with the Defence Research Staff, British Embassy, Washington, has been appointed to H.M.S. *Sultan*, Gosport.

R. L. Ninnes (*Fellow*) Special Director responsible for Technical Services, will direct the activities of Bristol Siddeley's European Headquarters in Paris to be established in September.

Techniques of Non-Destructive Testing

An evening course of eleven lectures on "Techniques of Non-Destructive Testing" will be held in the Physics Department of Brunel College of Technology, Woodlands Avenue, Acton, London, W.3, every Wednesday from 7 to 8.30 p.m., beginning 4th October 1961. The lectures will be on topics such as Radiology, Ultrasonics, Eddy Currents, Magnetic Methods, and Thermal Conductivity, and should be of interest to inspectors and others engaged in this work and also to graduates and holders of the H.N.C. in engineering and science subjects. The fee for the course will be £1. Details may be obtained from the Physics Department at the College.

The Journal of the Royal Aeronautical Society

INCORPORATING THE INSTITUTION OF AERONAUTICAL ENGINEERS AND THE HELICOPTER ASSOCIATION OF GREAT BRITAIN

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UPPER ATMOSPHERE RESEARCH

VEHICLE AND INSTRUMENTATION PROBLEMS

An All-Day Symposium on Vehicle and Instrumentation Problems in Upper Atmosphere Research was held by the Society on 11th March 1960 at the Institution of Mechanical Engineers, 1 Birdcage Walk, S.W.1. The Chair was taken by A. V. Cleaver, F.R.Ae.S., F.B.I.S., Chief Engineer (Rocket Propulsion), Rolls-Royce Ltd., Aero Division, and Chairman of the Society's Astronautics and Guided Flight Section.

Introductory papers were given by Sir Harrie Massey, F.R.S., B.A., M.Sc., Ph.D., Quain Professor of Physics, University College, London; J. F. Hazell, Guided Weapons Department, Royal Aircraft Establishment, Farnborough; Dr. R. L. F. Boyd, B.Sc., Ph.D., Reader in Physics, University College, London; Dr. G. D. Robinson, B.Sc, Ph.D., F.I.P., Deputy Director (Physical Research), Meteorological Office; H. J. Higgs, B.Sc., B.E., Australian Weapons Research Establishment Representative at U.K. Ministry of Aviation; and E. C. Cornford, B.A., F.R.Ae.S., Head of Guided Weapons Department, Royal Aircraft Establishment (now Chairman D.R.P.S., Ministry of Defence).

The discussion, which was in three periods, has been summarised by Miss B. E. Beadle, B.Sc., A.F.R.Ae.S., M.I.A.S., Technical Department, Royal Aeronautical Society.

Introduction

BY

Professor Sir HARRIE MASSEY, F.R.S., B.A., M.Sc., Ph.D.

(Quain Professor of Physics, University College, London)

THE LINE THAT I will follow in these few remarks is to say something about the requirements of the scientific user of high-thrust rockets and I will take what may seem the narrow view, of concentrating on that aspect. I am not suggesting that high-power rockets have only one function—to take up scientific instruments to make measurements of a physical, biological, or other scientific character—but I will concentrate on that side.

First of all, I think one ought to pay a little attention to the relative value of vertical sounding rockets as compared with satellites. In both cases, a large number of the observations which one wishes to make are observations of quantities which are highly variable in time and in space, and therefore it is obviously an advantage to use a satellite to make these observations, provided the satellite will stay up for a long enough time at the altitudes in which one is interested. The normal flight time of a vertical sounding rocket is, say, up to three or four minutes. Quite clearly then a satellite circulating for a month, with instruments aboard similar to those which are carried in the vertical sounding rocket, is potentially comparable to some eight to ten thousand such rockets from the point of view of the information it will give in a month, provided it continues to circulate. Therefore, from the point of view of the amount of scientific information which one gets back per unit of expenditure, obviously the satellite has a great advantage under these circumstances. On the other hand, of course, the rate at which one acquires information (let me be quite sure there has not been any misunderstanding—the rate at which one *ac*quires, not necessarily the rate at which one *re*quires information) is very much greater with a satellite than with vertical sounding rockets and so one has to spend money very much faster; and this may be, of course, in many instances the deciding factor as to whether satellites are launched or not. But if these factors do not come into it, then from the point of view of the information one gets from a given expenditure the satellite would be the more effective, and offers the great advantage of following systematic variation of the various quantities under observation. However, there is the important limitation that satellites, as they are launched these days, do not have a long lifetime if their perigee distance, that of closest approach to the earth, is less than a certain amount which we can fix, for the sake of argument, at about 200 km. If the perigee is less than this then a satellite will have a relatively short lifetime, owing to atmospheric drag, and the advantage of using it is largely gone. This means that, below 200 km., it is

necessary to use vertical sounding rocket methods despite the fact that one is dealing with highly variable quantities. One can therefore see a very long future in this work, involving years and years of observation as well as interpretation and analysis. It becomes clearly important to think of small, inexpensive rocket vehicles for frequent synoptic firings to cover the range up to 200 km. Above 200 km., satellites, apart from certain exceptions, will normally be the most effective vehicles for carrying out the scientific work.

Again, one must consider the important question, how far does one need to send equipment? This of course, is very much a matter of opinion, and depends on one's attitude to many things. There are, however, some perfectly clear scientific points which arise, and one is that a major return is realised immediately one is able to observe from outside the atmosphere or perhaps more accurately, outside the effective atmosphere. This is a very big dividend, because it not only makes it possible to look back on the earth, but it makes applicable whole new sciences of astronomy in the wave length ranges which are not capable of being tapped from the surface of the earth. Optical astronomy is confined to observations in that narrow range of wave lengths of visual radiation which reaches the ground. All astronomical science in the past, until the Second World War, was confined to working in this confined wave length region. Since the war radio astronomy has also developed, taking advantage of the other small windows at radio wavelengths but there is an enormous part of the electro-magnetic spectrum which cannot be employed for astronomical purposes if one is limited to ground observations. Once outside the atmosphere this enormous field may be tapped, and it is not necessary for the equipment to go far outside for this to be done.

However, if one considers the question of going further there are other points which come into the story. Of course one of the most interesting scientific discoveries through the use of satellites and deep space probes has been that of the radiation belts which extend outside the earth to distances of the order of fifty thousand miles, and which are obviously of much interest and importance in the study of the relations between the sun and the earth. The sun pours out charged particles into space at irregular intervals, depending on the degree of disturbance present in the solar atmosphere. These charged particles are affected by the earth's magnetism, and this is quite a complicated interaction. It is a two-way interaction, because the magnetic field of the earth, at considerable distances from the earth, is drastically altered by the streams of charged particles. So there exists a complicated interaction, which has quite marked effects even for us on the ground, in that it produces auroral displays, magnetic disturbances (magnetic storms) and disturbances of the ionosphere which make for instance, radio telephony between the two hemispheres of the earth difficult. All these matters are of great interest and importance to study and they involve of course, deep space probing—it is necessary to send out instruments to distances of tens of thousands of miles to study the radiation belts.

In addition to this, of course, there is the interest in studying the moon and the planets. A great deal exists to be explored here. And of course as those who speak about the scientific side are usually physicists, they tend to play down or even ignore, any biological aspects of the work, which is unfortunate. It is true, I think, that space biology as such does not need, or does not provide, any environment which cannot be simulated on the earth, except that of gravity-free conditions. But there is the very important and interesting problem of whether or not there is any semblance of biological organisation on the moon and the planets. This is not by any means a purely sensational question, because the subject of biology at present is in a very interesting stage. Biophysics, at any rate, is perhaps the subject most likely to expand to an almost explosive extent in the next few years, and therefore there will arise many fundamental problems in biology for solution. These include such questions as to whether or not any kind of life, or anything which we would tend to call life, could develop on any other chemical basis than that on which it has developed on this earth, namely the two nucleic acid molecules DNA and RNA. These seem to be the vital basic necessities for the construction of life on this earth of all kinds, and biologists would naturally like to know whether or not this is an essential requirement, or whether it is possible to build life of some kind on some other chemical basis. It would therefore be of great interest to examine the situation on the planets, to see whether any such development has actually proceeded. Equally interesting would be the investigation of whether anything similar at all to ordinary life, based on DNA and RNA, has been able to develop on Mars and Venus, for example. These are big, important and fundamental questions, and there is no denying it would be of great interest scientifically, and generally, to obtain answers to them. This poses difficult problems of technique because at least for quite a long time to come, the first efforts to solve these problems will have to be done automatically, in the same way as the measurements that are being made of physical quantities; but this is rather more difficult in the biological field, and poses very severe problems of design and interpretation.

There are clearly quite a wide range of scientific problems to study. One can get a large dividend in the physical sciences by merely going outside the atmosphere—"merely," I use in a purely relative sense—but there are other things of much interest which one can hope to get out of more distant journeys. All this is possible even without any question of manned flight as these observations can be made automatically, with varying degrees of difficulty. The biological ones may be the most difficult from this point of view.

Now, given that there is this wide range of possibilities for scientific work, what are the conditions under which the work may be carried out? What does one require of the vehicle? The first essential is that it should take the instruments where one wants them to be taken, and this means, of course, that it should have the necessary propulsion to take it far enough, and the necessary guidance in some form or other to take it in the right direction and with the right speed. These are obvious basic requirements which quite clearly can be fulfilled,

provided one spends enough effort. In addition to this there is the necessity of being able to follow the path of the vehicle very accurately. A lot can be obtained from increased accuracy and this particularly applies to nearer satellites, for example, from which we can earn a great deal of extra dividend from an increased precision in tracking. And then there is the very important necessity that not only should one be able to track, but one must also be able to recover data from the observations made by the instruments in the vehicle, and this has to be done in many instances over great distances. One cannot always arrange for a vehicle, which is to pass to great distances from the earth, to come back to a close point to relay its information. If this can be done it is a real advantage and I understand that the Russians, in their third Lunik, which photographed the back of the moon, did intend to make their major television transmission when the vehicle was much closer to the earth than the first transmission, which was from a very great distance; but something went wrong, and so they were unable to get this closer transmission. This is just a sample of the sort of things that happen. It was a real advantage that they were able to get something back even from the great distance, and one must hope for developments which will enable work to be done more effectively from very distant transmission.

These are quite general requirements. If we consider the question of astronomical observations, which, as I indicated, offer perhaps the widest scope for new scientific work, it is clearly an advantage to be able to have in a satellite a platform, stabilised in respect to space, so that the instruments in the satellite can be oriented precisely to observe particular astronomical objects. As in all these things, one can proceed further and further in the direction of more and more precise stabilisation, but it is not necessary to go to the utmost limits of precision in the first instance. It is a new field, and one can penetrate it gradually. Once a space-stabilised satellite is available, then one can enter the astronomical field in a very satisfactory fashion. One must also, in this connection, think of other possibilities which one would like to be able to do. Naturally one tends to think about the other end of the spectrum, from the point of view of astronomical observations, the long wave radio region, where there is a cut-off due to the ionosphere. Arrangements are being made for observations of the total intensity of the radiation in this wavelength region from the galaxy to be measured in the fairly near future. But this does not provide a great deal of interesting information. What one wants to do is to find out where in the galaxy the radiation comes from. This is difficult at these long wavelengths, as one needs enormously large aerials for this purpose, and at first sight this would seem to be pretty impracticable on a satellite; but it has been pointed out by those who have had the trouble of building the large radio-telescope at Jodrell Bank, that there are certain advantages in putting up large aerials in a region where there is zero gravity. It is not by any means entirely out of the question to spread out a large aerial from a satellite so that directive observations may be made even in this long wave radio region. This is the sort of thing that the radio astronomers, who are interested in extending their wavelength range are beginning to think about.

There is another question concerning research with satellites and in fact with all space vehicles, including vertical sounding rockets, and that is, whether it is of any value to the scientific work to have recovery—whether it is necessary actually to recover the instruments, or not. For many purposes the answer is No! There is hardly anything now that one cannot transform into electrical signals for transmission by telemetering link. But there are some occasions when it is an advantage to be able to recover. Quite often very much more information can be obtained if a photographic plate, or even a stack of photographic plates, can be recovered, as in cosmic ray work. But there are quite considerable limitations involved in this. Thus in principle it would be excellent to be able to fly a large stack of emulsion—nuclear-sensitive emulsion, as it is called—a really good big stack, in a satellite, expose it to the cosmic radiation over the orbit of the satellite, and then, after a suitable lapse of time (which is not so long that the emulsion is completely fogged), arrange for it to be sent back to earth in a state where it can be recovered for processing and analysis. If it could be got back to earth in such a convenient state, then it could occupy practically all the groups in the world which are concerned with the analysis of photographic emulsions of this kind exposed to cosmic rays, for quite a while. There would be an enormous lot of interest in the stack. But it is a big technical problem to get it down through the atmosphere without having it overheated and so on.

On the whole, however, the requirement for recovery is not so very strong. It is one of these things that, if asked one can think of applications, but one does not normally say "Oh! I can't possibly carry on without having recovery." Perhaps this might be true for those who are concerned with the use of photographic emulsions, but otherwise it is usually possible to get by in some other way.

There are, in this Symposium, papers discussing the various aspects of some of the vehicles which are being used, and when it comes down to a slightly less general level from which I have been talking, there are many things of great importance which have to be taken account of. One of these is quite vital, and that is that one must be quite certain, when making observations of some physical quantity from a vertical sounding rocket, that one is measuring the quantity as it would be if the rocket were not there. It is very important indeed to make sure of this as the rocket is quite a substantial disturbance to ambient conditions. One has to be very careful that the disturbance is not so great as to mask the undisturbed quantity itself. There have been many instances in the past where this has given rise to great difficulty in interpretation of data. For example, in the first observation of the composition of the atmosphere, what was actually sent for analysis was ground air which had been taken up by the sampling bottle. There have been observations of ionisation in the upper atmosphere which one suspects were associated with rocket gases arising in some way from the rocket fuel, rather than from the actual atmosphere itself. And there

are many other occasions of this kind. Care is particularly necessary with vertical sounding rockets because with a satellite there is more time for things to settle down.

It is clear that when the first data from any scientific experiment are obtained that care must be taken to check that what one has got is in fact what it was planned to measure and not something that was only put there by the rocket itself. Of course it must also be remembered that this does raise the possibility of being able to do experiments where the ambient conditions are modified in a controlled fashion, and the results observed. There have already been experiments of great interest and importance carried out in this way. For example, there have been the so-called "seeding" experiments, where sodium vapour or nitric oxide or other substances are injected into the upper atmosphere from a rocket and the effects observed from the ground; and then of course there have been the largest-scale experiments ever carried out by man—and carried out successfully—where artificial radiation belts were created round the whole earth by injecting into the earth's magnetic field charged particles from an atom bomb explosion at an altitude of roughly 300 miles. There is very big scope indeed for upper atmospheric and cosmic experiments.

Skylark

BY

J. F. HAZELL

(Guided Weapons Department, R.A.E.)

SKYLARK is an upper atmosphere rocket designed to carry a payload of 150 lb. to a height of 100 miles. The project was the outcome of discussions between the Royal Society and the Ministry of Supply which started in 1953 and were completed in 1955 when Treasury approval was given for a joint Royal Society–Ministry of Supply programme of upper atmosphere research using a vehicle to be designed by the Royal Aircraft Establishment. The programme was the responsibility of the Gassiot Committee of the Royal Society, matters of detail being agreed at a working party and endorsed by the Gassiot Rocket Sub-committee.

Arrangements for British participation in the International Geophysical Year had been made before this project had been agreed so that no rocket programme was included but an ambitious target date of summer 1956 was set for the first proving rounds in the hope of making a useful contribution.

The Vehicle Design

The basic vehicle layout is shown in Fig. 1. The largest and most important part is the Raven solid propellant motor, of nominal diameter 17 in., which gives a thrust of nearly 12,000 lb. for 30 seconds, using a composite propellant of ammonia perchlorate, ammonia picrate and polyisobutylene. This was the largest motor of its type to be attempted in this country, at the time, and made Skylark the first single-stage high performance solid propellant sounding rocket in the world. Although some development was involved in producing this motor the aim was to keep the vehicle design as simple as possible to ensure an early start to the programme. The solid propellant motor simplified the handling and ground facilities, removed the problems of staging and kept dispersion and safety problems as simple as possible.

The instrumentation compartment is a conical nose

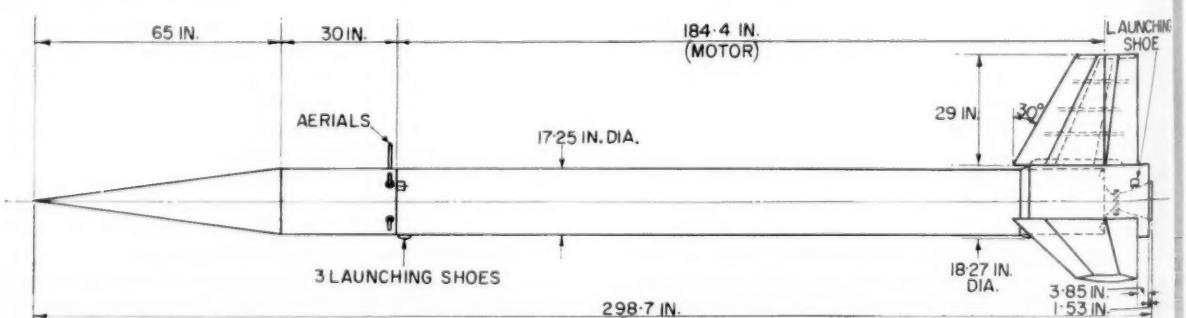


FIGURE 1.

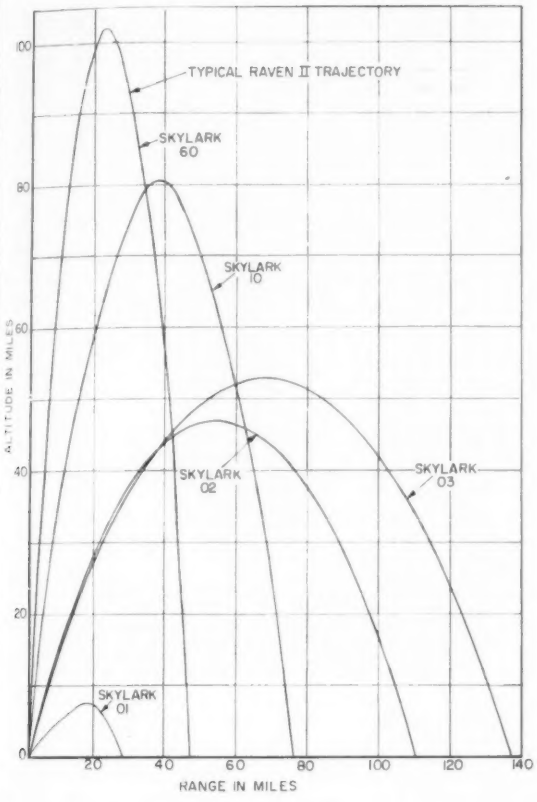


FIGURE 2.

of 7½° semi-angle on a cylindrical body casting of magnesium alloy giving a payload capacity of 5 cu. ft. The vehicle is stabilised by three fins at the rear carried on a casting which fits as a unit over the motor tube.

The weight break down, in round figures, is:—

Propellant	1800 lb.
Motor case	400 lb.
Fin assembly	100 lb.
Forward structure	100 lb.
Payload (instrumentation) ...	150 lb.
	2550 lb.

Performance

Typical trajectories are shown in Fig. 2.* The range to impact is about 15 miles per degree from the vertical. This can be reduced to about 10 miles per degree by increasing the thrust at launch at a cost of about 15 per cent in altitude. The range could not be reduced much below 8 miles/degree for a 100 mile altitude trajectory, irrespective of motor thrust, without some form of control during flight. Skylark is normally fired with a ground range of 50–80 miles.

Changes in payload produce changes in height of about 800 ft./lb.

*Skylark has since been fired with a Cuckoo boost. Skylark number 67, for example, reached 150 miles.

Above about 40 miles a vacuum trajectory is a satisfactory approximation, so the flight duration in any particular altitude bracket can be determined from the familiar relation $S=\frac{1}{2}gt^2$.

Environment

By rocket standards, instrumentation in Skylark has a comfortable ride. Acceleration due to motor thrust is less than about 10g, and the vibration level is low. Temperatures inside the vehicle range from 30°–50°C while outside the skin temperatures are reasonable for probes and aerials. The payload space is usually pressurised.

Dispersion and Launcher

The Skylark launcher, and a typical launching are shown in Figs. 3 and 4. The launcher, which is 100 ft. long is robust and accurately made from Bailey Bridge panels. The main reason for this considerable length is to give a reasonable launch velocity (about 150 ft./sec.) and so reduce the effect of jet malalignment and wind. Even with this launch velocity a 15 ft./sec. wind, for example, means that the rocket would leave the launcher with an incidence of 1/10 radian to the relative wind and aerodynamic stability would then cause the vehicle to turn by nearly this amount into the wind. The launch direction therefore needs correction by a similar amount —for which purpose remote control of the launcher is provided—and if the corrections were ignored in this example an impact error of 80 miles would result. Because winds are so important they are carefully measured before flight and special weighting factors applied to enable accurate correction to be made.

Fig. 5 shows the launcher chart on which these corrections are made and the required azimuth and elevation angles transformed to launcher gimbal axes. The concentric circles represent equal angles from the vertical and the radial lines azimuth direction. The grid of launcher gimbal angles α and β is nearly Cartesian.

The required impact point, P, is plotted on this chart at the elevation corresponding to the range under still air conditions. A small correction can be made for the Coriolis displacement but this is usually ignored. From the point P is drawn a vector PA in the direction of the weighted wind using a scale specially calculated for the purpose (about ¼°/ft./sec.).

The required launcher setting is indicated by the point A, and this can be read off on either set of axes. The launcher setting can be conveniently plotted at intervals before firing to indicate the trend of the wind correction.

Instrumentation and other facilities

Most Skylarks are flown with a range of standard instrumentation which includes a 465 Mc/sec. AM/FM telemetry sender which permits information on 23 channels to be sampled 80 times per second on each channel. Trajectory and velocity information are provided by a 104–208 Mc/s doppler system, and a 6 cm. tracking beacon, the latter using suppressed aerials. Linear accelerometers, rate gyroscopes, magnetometers, pressure gauges, sun attitude devices, temperature

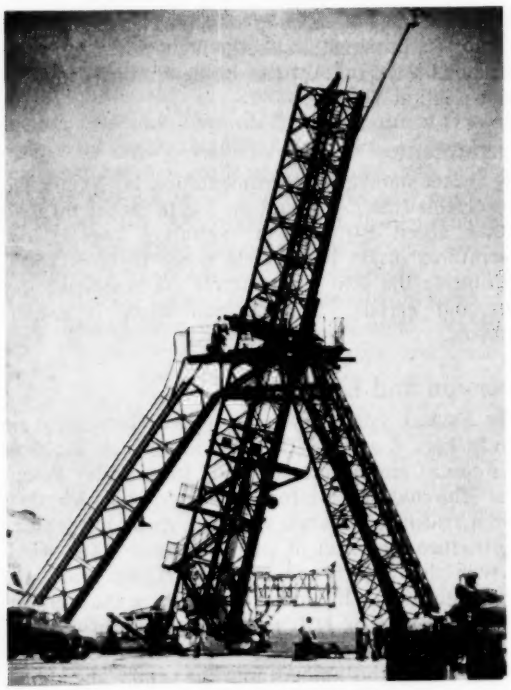
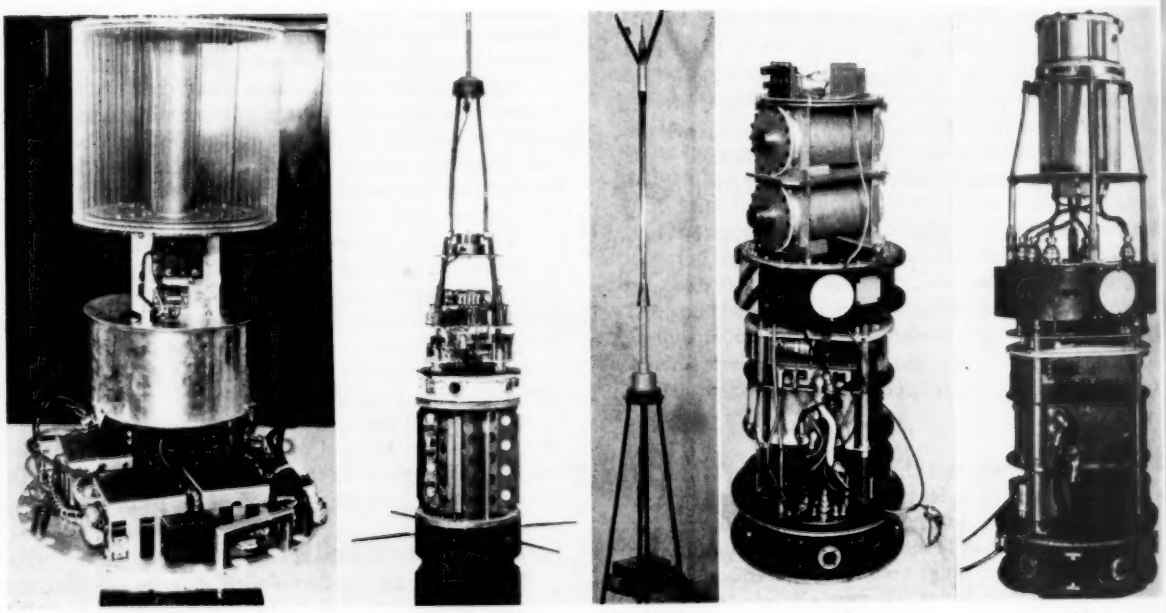


FIGURE 3. The Skylark launcher.
(The beam is now 100 ft. long, not 80 ft. as shown.)

FIGURE 4. A Skylark launching.



(*a*) (*b*) (*c*) (*d*) (*e*)

FIGURE 6. Typical Skylark installation.

(*a*) The Birmingham Ion Cage. The nose cone is first removed and the ion cage then ejected on a hundred feet of cable by means of a coiled spring. (*b*) Grenade and Window Bay (University College and Imperial College) and Birmingham University Electron Density experiment using an insulated nose cone. (*c*) A sting mounted ion probe flow by University College, London. (*d*) Sodium Vapour dispensers used by The Queen's University of Belfast. (*e*) An Airglow Photometer installation of The Queen's University of Belfast. Part of the nose cone is ejected forwards to expose the photometers to the sky.

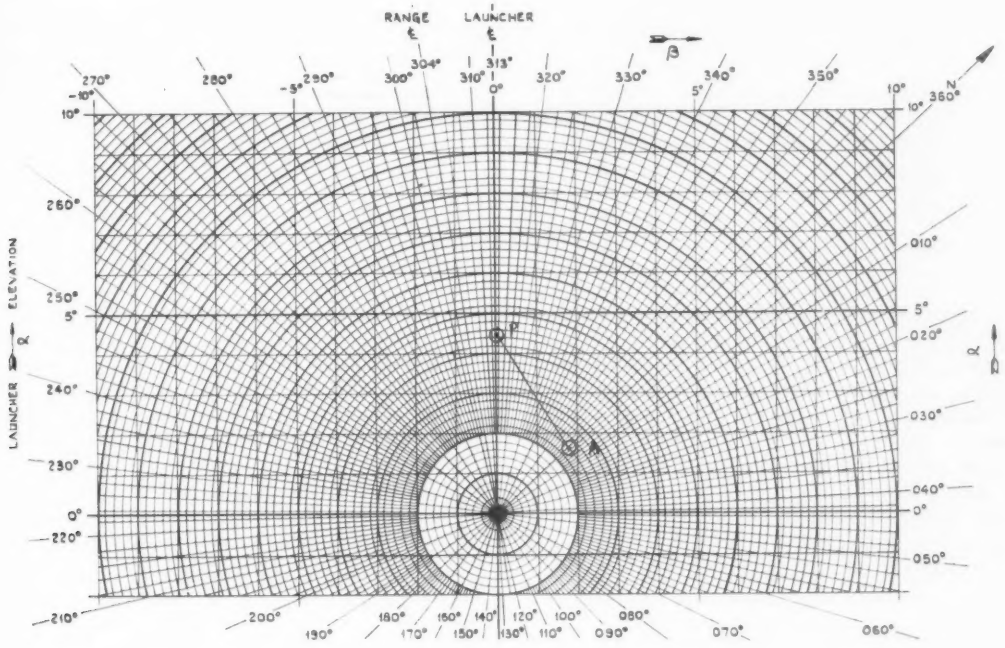


FIGURE 5.

gauges and programming and sequencing units, etc., are usually carried.

Part of the nose cone has been projected gently forward for airglow photometer and electron density experiments. A split nose cone will be used in a stellar ultra violet experiment, and many others. The whole head can be separated from the motor tube to avoid contamination—and also to aid recovery. Long probes have been carried ahead of the nose cone, and a large variety of devices carried in the skin of the round.

A parachute recovery system has been designed to give a descent rate of the whole head of about 25 ft./sec. which will enable recovery of almost any equipment in an undamaged condition. Without parachute the separated head impacts at about 250 ft./sec., with severe damage. A drogue, released at about 15,000 ft. altitude (and terminal velocity 115 ft./sec.) reduces the speed and deploys the main 20 ft. diameter canopy.

In the grenade experiment a special installation is used to eject eighteen grenades through the skin of the round and two batches of window cartridges. This experiment has been combined with the sodium vapour experiment where two large sodium dispensers are carried in a forward position.

Also under development is a pneumatic stabilisation system which will maintain the whole head in a predetermined position with an accuracy of about $\pm 2°$.

Typical installations are shown in Figs. 6 (a) to (e).

A pitch-yaw-roll coupling problem

Once through the transonic range the roll rate of Skylark tends to increase steadily during motor burning due to the roll misalignments which are always present. The pitch frequency increases to a maximum at about 15 secs. and then falls as $\frac{1}{2}\rho V^2$ diminishes with increasing altitude. At some time during flight, therefore, the roll and pitch frequencies are likely to coincide and this can lead to an undesirable amplification of misalignment effects.

The way this arises can be seen readily by making the simplifying assumption that at the height, speed and roll rates concerned the gyroscopic effects can be ignored in comparison with aerodynamic terms. Thus it can be assumed that the misalignments alone rotate at the roll rate rather than the whole missile. Under these conditions the damped spring system which represents the weathercock motion in one plane will be forced by sinusoidally applied misalignment moments with the well known consequences to phase and gain, the gain at resonance being severe owing to low damping at altitude.

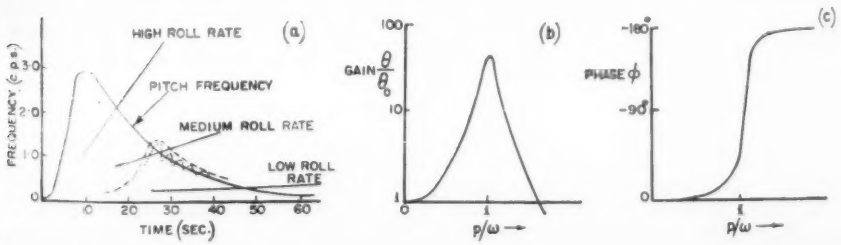


FIGURE 7 (*a*), (*b*) and (*c*).

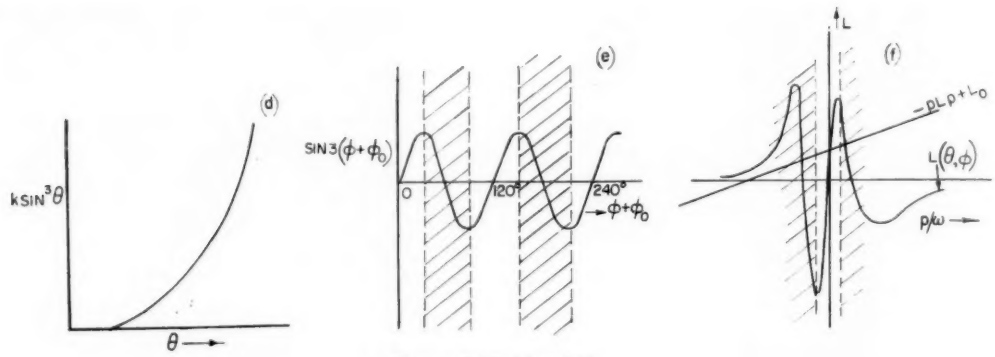


FIGURE 7 (*d*), (*e*) and (*f*).

Fig. 7 (a) shows a typical variation of pitch frequency versus time.

For a high roll rate resonance occurs at high $\frac{1}{2}\rho V^2$ and break up due to high acceleration is probable. For a medium roll rate, resonance occurs later in flight but high incidence can still develop during motor burning resulting in low performance due to the angle between the thrust and the flight path as well as to induced drag. In the case of a very low roll rate, resonance will have negligible effect on the trajectory but high incidence may still occur, leading to a tumbling motion which is unsatisfactory for some experiments.

Ill effects from this type of resonance, which occurs over a limited part of flight, could be avoided without difficulty by careful attention to fin alignment and so on, if it were not for a more serious effect which causes the roll rate to increase above the expected trend and to lock in at resonance (sometimes in an oscillating manner) as sketched in dotted curves in Fig. 7 (a). A simple explanation of this behaviour is attempted below.

Suppose the lateral misalignments produce an incidence, θ_0, in a particular missile plane, ϕ_0, when the roll rate is zero. When the vehicle rolls this incidence is magnified by the gain and the plane changes through the phase angle as sketched in Figs. 7 (*b*) and (*c*) which are normal damped spring system response curves. For a given frequency ratio the incidence, θ and phase angle ϕ can be determined from these curves. Now there is a rolling moment which results from asymmetric flight which can be represented approximately by the expression $L(\theta,\phi)=k\ \sin^3\theta\ \sin 3(\phi+\phi_0)$, where k is a coefficient depending on Mach number, etc. and ϕ_0 is the angle of the misalignment plane from a fin. The rolling moment due to this coupling can therefore be obtained by multiplying values read off from the two curves, (Figs. 7 (*d*) and (*e*)), and these can be determined for any frequency ratio from Figs. 7 (*b*) and (*c*).

In addition to this rolling moment there is a damping moment pLp and a misalignment moment L_0. Equilibrium conditions are given by the intersection of the coupling moment $L(\theta,\ \phi)$ with the straight line $(-pLp+L_0)$ versus p/ω, at which points the total rolling moment is zero.

Because the response in roll is fast compared with pitch it is clear that a perturbation in roll will occur at constant θ so that stability depends on the shape of the sin $3(\phi+\phi_0)$ curve. The stable regions are shaded on Figs. 7 (*e*) and (*f*). These figures are continually changing with time as the speed and density vary during flight.

It is evident from this treatment that if the vehicle were to spend long enough at a particular height and velocity a stable position near resonance would be reached. More detailed calculations show that results given by the simple treatment are very close to the actual motion under conditions of changing altitude and speed. Moreover, the oscillating motion referred to earlier is reproduced qualitatively, but the theoretical calculations do not predict the severe resonance difficulties that have been experienced in flight unless the misalignment torques or the aerodynamic cross couplings are much larger than our estimates, so that the phenomena require further investigation.

Concluding remarks

Although the first Skylark launching on 13th February 1957 was rather later than the optimistic target that had been set, a considerable number of experiments were attempted during the International Geophysical Year and this was more than had been expected; indeed Skylark was one of the few vehicles to be fired during the international rocket weeks.

The performance achieved has been very close to expectations, and far more than originally asked for by the Royal Society. There have been failures and disappointments—as in any rocket programme—but the results achieved are most gratifying, and the growing experience of all the teams concerned should ensure even greater success in the future.

Acknowledgments are made particularly to the Rocket Propulsion Establishment (Westcott) for the motor design, to Bristol Aerojet Ltd., and Hudswell Clarke & Co. Ltd., for the motor and vehicle hardware, to E.M.I. Electronics Ltd., who were responsible for the telemetry, and finally to the Weapons Research Establishment in Australia for their impressive co-operation in carrying out all the actual firings.

The Scientific Programme of Skylark to Mid-1960

BY

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(Reader in Physics, University College, London)

TODAY WE ARE INTERESTED in the scope of the research programme rather than the actual details of the physics. Further I shall discuss space research rather than just upper atmosphere research, because we include astronomy and this will be discussed by others as well.

Now I have, in fact, a formidable task, because I find I have something like twenty-three experiments to deal with in twenty minutes. What I am going to do therefore is to list the experiments that are going on, to tell you whether they have been carried out successfully, or are awaiting completion, and thus to try to give you some idea of the scope of the work which has been undertaken. And at this stage I would like to say how very much we are indebted to many collaborators, but especially to Mr. Hazell's team. Mr. Hazell and his co-workers have made possible this programme, often I think in the face of the demands of much higher priorities at the Royal Aircraft Establishment, and sometimes at W.R.E. I think you will agree when you consider this, and also our rather small budget, that we have made a considerable attack on the subject on a wide front.

Most of the experiments that have not yet been flown are in a very advanced stage of preparation and should be flown quite soon; they are in fact scheduled for flight.

The first experiments are those concerned with the gross physical properties of the upper atmosphere. By which I mean not such subtleties as the degree of electrification, or photo-chemistry, but things like temperature, motion and so on. The first experiment, the grenade experiment, is aimed to measure wind, to heights below 90 km. The idea is to measure the velocity of sound which is in fact the most reliable way of determining temperature and the motion of the atmosphere in this height range. It has been successful. By a very kind act of nature, which I think is quite exceptional, we discovered that grenades would provide data on wind above 110 km., because the products of the explosions glow photo-chemically, and their motion can be tracked.

Next we have the sodium trail experiment which has been used primarily to give wind at heights greater than 60 km. It has also been successful. The wind is obtained by observing the motion of the cloud, which fluoresces in the sunlight after the sun has set on the lower part of the atmosphere. The temperature may be obtained from a measurement of the sodium resonance D lines by an interferometer.

Wind is also measured by the Window experiment. Window, you will recall, is the name given to those tiny metallic dipoles which are ejected as an anti-radar device. In this case they are projected from Skylark, and tracked by radar, in order to observe the wind motion. It has been used quite successfully, and probably some data on turbulence may be obtained from it. Incidentally, sodium will also give some data on turbulence.

A sphere-drop experiment is planned. It gives density above 100 km. The maximum height depends very much from what height you may drop the sphere. The drag on the sphere is measured, in our case, by a photo-electric tracking method developed for satellite tracking, which gives precisions of the order of one second of arc in position.

Finally, for the gross properties, microphones are being used to detect the meteoritic dust.

There has been much interest in the ionosphere. We recall that major contributions to the study of the ionosphere in the past, before the age of space research, were made in this country. The dielectric constant probe has been flown successfully to give electron density by observing the changes in the capacity of the region around the vehicle, as measured between electrodes which are powered with a radio frequency. There is a highly dispersive relationship between the capacity and the frequency, and if one uses frequencies near the critical frequency for the region there is a big change in capacity due to the ionisation which can be measured by its effect on the frequency of an oscillator tuned by the capacity. This experiment is being prepared for the first Anglo-American Satellite.

The Langmuir probe has been flown successfully. It is aimed primarily to determine the concentration of the heavy negative ions in the atmosphere, a problem still by no means well understood, and for which only poorish figures, sometimes only guesses, exist. The experiment operates by determining ion and electron densities from the current-voltage relation of the probe.

We also have a device which we call a sporadic E probe, which is a simplified Langmuir probe, designed primarily to look for rapid changes in the ion density such as are obtained with the sporadic clouds of ionisation that often occur around 100 km. This experiment has been flown successfully. We find too that it is quite a useful way of getting the profile of ionisation in the ionosphere even though the changes are not rapid. Although originally devised to look for sporadic E ionisation it is being used now to look at the ionosphere profile.

An experiment of remarkable complexity and ingenuity which has also been successful is the ion cage mass spectrometer, a piece of instrumentation fired out on the end of a cable some hundred feet long to make sure

that it is well away from the rocket. Good mass spectra confirmed that nitric oxide is an important ion in the upper atmosphere. The device works essentially by measuring the rate of passage of ions between a cylindrical grid and a collector which is in the centre of the cage.

There are two experiments on the ionosphere based on the propagation of radio waves, one on the variation of the group velocity of a radio wave, and one on the variation of the phase velocity of a radio wave with electron concentration. Neither has yet been tried, but they soon will be.

There is an experiment on electron temperature, a special version of the Langmuir probe, as one of the experiments devised for the first Scout satellite.

We are working in the laboratory on a little mass spectrometer with an extremely small path length so that it will work in relatively high pressures. I hope one day it will be used to study ionic constitution in the D-region.

There are experiments on photo-chemistry photo-multiplier tubes, arranged to observe the green line of oxygen at 5577 Angstroms and red line at 6300 Angstrom units. The instrumentation has been flown and tested successfully. Other flights are scheduled in the near future.

Proton-precession magnetometers, aimed primarily to look at the currents in the upper atmosphere in the E-region will be flown soon.

Now to leave the earth and go out into space, we have astronomical experiments.

Emulsion detectors for soft X-rays using various foil filters have given fluxes typical of quiet sun conditions. This experiment tells us how the outer part of the solar atmosphere, the solar corona, is behaving when we do ionospheric experiments, because although a lot of work has been done on the sun and a lot on the atmosphere, there has not in the past been that emphasis on doing the two at the same time which is necessary to give a good correlation between the behaviour of one and the behaviour of the other so as to investigate the solar control of the ionosphere.

We have successfully flown ion chambers looking for Lyman—α radiation, the resonance radiation of hydrogen which is a good measure of the way the sun is behaving, not so much in the corona, but a little lower down, in the chromosphere.

We also have proportional counters for X-rays. These give the spectral distribution in the range ! to 20 Angstroms. They are scheduled for flight very shortly. This experiment is also being prepared for the first Anglo-American Scout satellite.

We have two stellar-type astronomy experiments being prepared. In the sky scan experiment, we scan the sky in ultra-violet light (1700 Å) using photo-multipliers. The sky photography experiment uses the stabilised nose-cone to which Mr. Hazell referred, and looks especially for stellar images, for point sources of ultra-violet light (1700 Å).

Meteorological Measurements in the Upper Atmosphere

BY

G. D. ROBINSON, B.Sc., Ph.D., F.I.P.

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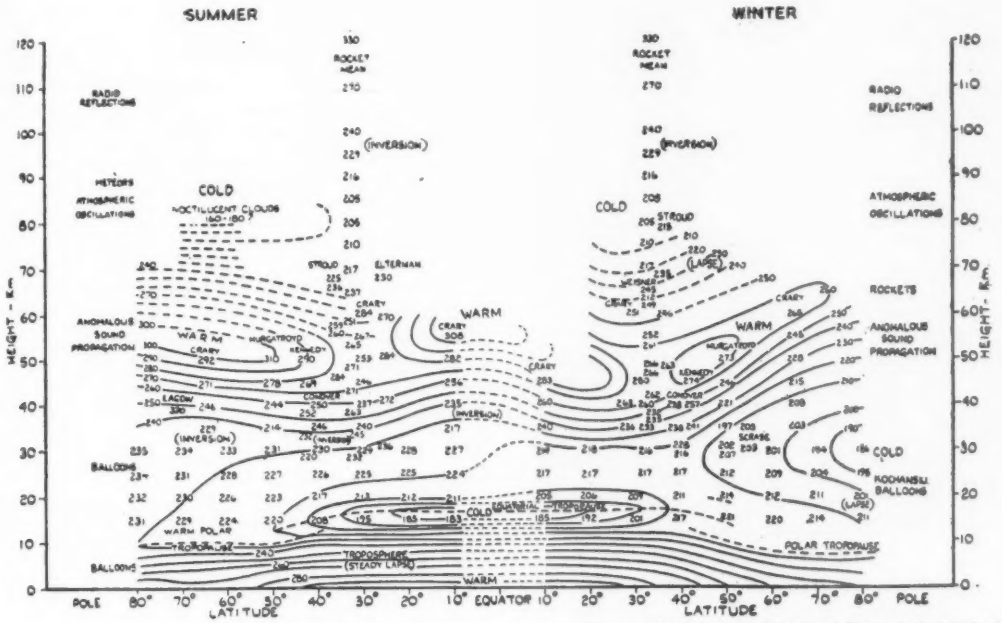
1. Introduction

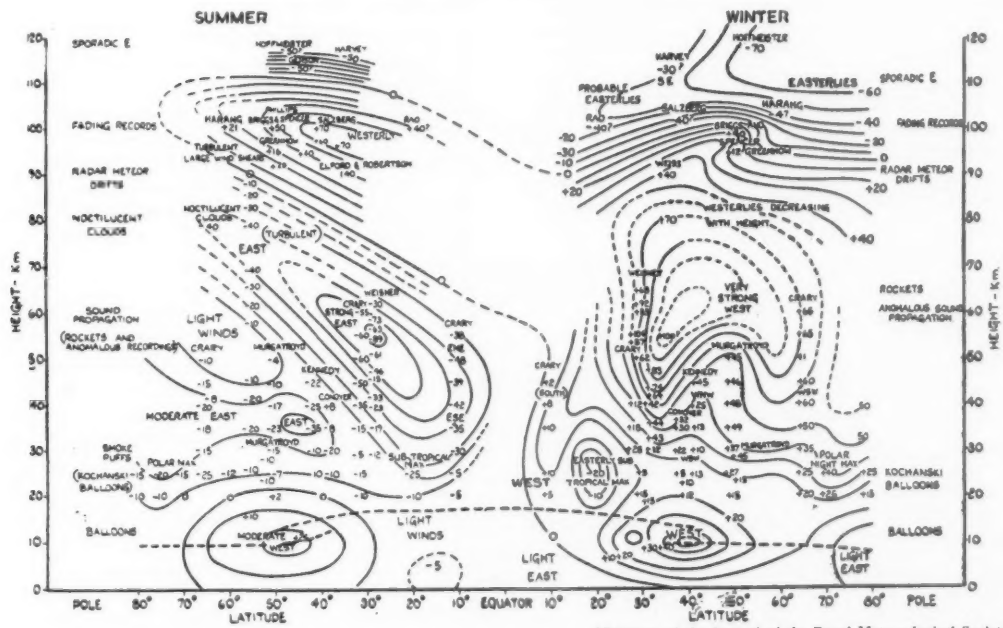
It should not be necessary to explain the presence of a meteorologist at a symposium on any aspect of upper atmosphere research. Meteorologists understand their science to comprise the dynamics, thermodynamics and chemistry of atmospheres, wherever atmospheres occur. They are happy to have the facilities for observation and even experiment in the higher layers of the earth's atmosphere which rocket and satellite vehicles begin to provide, but can point to a considerable body of existing knowledge obtained by other means. A review by Murgatroyd[6] of material available up to 1956 is perhaps the most convenient summary of this knowledge. Figs. 1 and 2 are reproduced from Murgatroyd's article. They are not entirely self-explanatory, and they do include a few data obtained by the use of high altitude rockets, but they should sufficiently illustrate the point that we are far from being dependent on such vehicles for knowledge of the inter-related fields of temperature and motion at least up to heights of 100 km. It follows that in this region the meteorological use of expensive vehicles should be planned with some care if it is not to be wasteful, and should either check those existing methods or assumptions about which there is some doubt, or observe in a domain of time or space, or to a degree of precision, not accessible to indirect methods.

The chief gaps in the mean charts of temperature and wind are in tropical and polar latitudes, and do not imply any fundamental failure of indirect methods in these regions. Results obtained during the I.G.Y. will probably improve the position. Investigation of the variability of wind and temperature in the upper atmosphere has, however, hardly begun. Regular tidal and seasonal variations are known to exist, and there is every reason to expect turbulent motion on all scales. A recent paper from Jodrell Bank[2] contains the first clear indication from about 90 km. of turbulence on a scale



(Courtesy of the Journal of the Royal Meteorological Society)

FIGURE 1. Observed atmospheric temperatures (°K), according to Murgatroyd (1957), showing the method used and the name of the investigator.



(Courtesy of the Journal of the Royal Meteorological Society)

FIGURE 2. Observed zonal component of atmospheric winds (m. sec^{-1}, Westerly component positive) according to Murgatroyd (1957), showing the method used and the name of the investigator.

comparable with that of the weather systems of the lower atmosphere, and turbulence on a smaller scale is evident, for example, in the photographs of sodium trails obtained in the course of the Skylark programme. The variability is most evident in the wind field, but it cannot be satisfactorily described, still less explained, in terms of the wind field alone, and we are thus led to an estimate of the required accuracy of density and temperature measurements. The extreme variation of density at the surface is within a range of about 10 per cent and the

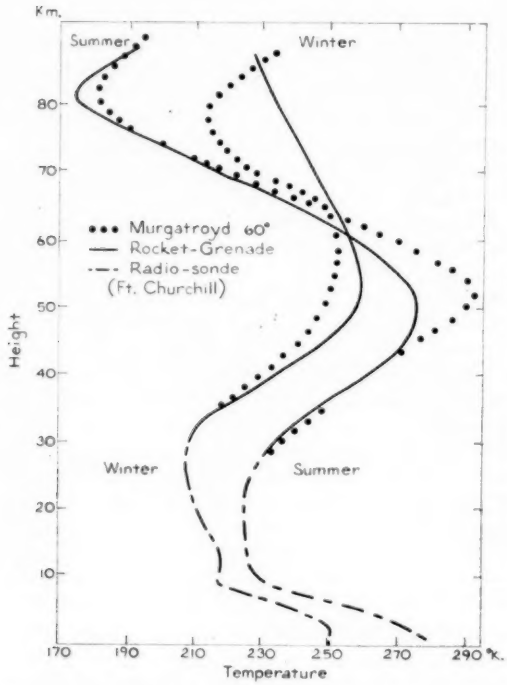


FIGURE 3. Atmospheric temperatures above Churchill, Canada (58°N) from grenade experiments mounted on Aerobee rockets during the I.G.Y., compared with Murgatroyd's (1957) estimates for 60°N.

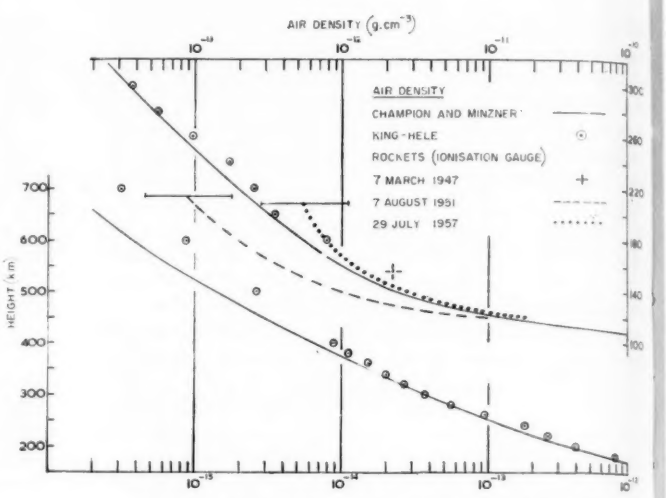


FIGURE 4. Mean density of the upper atmosphere. Champion and Minzner's curve above 170 km. is based on satellite data, below 170 km. on rocket data.

immediate aim in measurements designed to clarify the thermodynamics of the upper atmosphere must be for two or three per cent accuracy in density and 2°C to 3°C in temperature. This will not be easy to attain, either by rocket-borne instruments or indirect methods.

2. Meteorological Investigation by Rocket

The available methods have been touched on in the summary of the Skylark programme presented to this symposium. The most important are direct measurements by gauges, the "grenade method" using the velocity of sound, the observation of the motion of and radiation from atmospheric contaminants, and the "falling sphere method" in which the trajectory of an object is carefully observed and density derived from the drag.

Little has been published recently concerning the direct determination of density by gauges (developed from laboratory ionisation and Pirani-type vacuum gauges) carried in the body of the vehicle. The difficulty is to eliminate the effect of the vehicle's motion and of gases carried up by it. Early density estimates obtained in this way were uncertain by a factor of 2 to 5, but higher accuracy is probably now attained. In the layers below about 60 km. attempts are being made at direct temperature measurement using parachute drop-sondes released from rockets, but no results have been published.

The grenade method has been refined and used with considerable success by G. V. Groves in the Skylark programme, and by a N.A.S.A. group under W. G. Stroud operating from Churchill, Canada. Fig. 3 shows the mean results obtained by Stroud's group during the I.G.Y. period, compared with Murgatroyd's 1957 standard atmosphere for the latitude of Churchill. More significant for meteorologists than the changed mean values are some of the details of individual soundings. For example all winter curves showed one or more temperature inversions above the main inversion at 30 to 50 km., and two ascents in late January showed anomalies in the wind field at all levels, associated with a sharp temperature increase in the lower stratosphere—the first definite observation of physical linkage over such a depth of the atmosphere[7,8].

The introduction of contaminants into the atmosphere is a powerful method of observation, and Dr. Boyd has mentioned three examples by which winds have been determined in the Skylark programme—radar "window", the sodium vapour trail and the NO–O glow of the grenades above about 100 km. A recent American observation of the motion of a sodium vapour trail in the 200 km. region is noteworthy, since the scattering sodium atom is a neutral particle and one difficulty in interpreting observed motions in this region of the atmosphere is to isolate the electrically generated motion of ions from the thermally and frictionally driven circulations. The American observation is a clear demonstration of the movement of neutral matter at speeds (*ca.* 200 m. sec.$^{-1}$) previously attributable with certainty only to electric charges.

An elegant variant of the falling sphere method of density determination has been used below 100 km. by a team from Ann Arbor University[3] but it is likely that the chief application of the basic method will be to higher layers where other methods fail. The principle of the method is of course the same as that used in determining density from observation of satellite orbits.

3. Air Density and Satellite Orbits

Several authors, notably King-Hele[5] and Champion and Minzner[1], have investigated the derivation of

atmospheric density by determination of the drag on artificial satellites from careful observation of the rate of change of period. It is necessary to ignore electrostatic and electromagnetic accelerations; this is plausible though the effects are extremely complex and not fully understood. Fig. 4 shows some mean results of King-Hele and Champion and Minzner. The early rocket results which are included in Fig. 4 illustrate both the order-of-magnitude agreement with the deductions from satellite motions and the serious imprecision of the earlier work. The most interesting aspect of the "satellite" densities is perhaps not the mean curve, but the considerable variation at a given height and location which begins to appear, and which seems to be correlated with the period of solar revolution and, less surprisingly, with whether the atmosphere is sunlit or in shadow. It appears that we are beginning to observe the meteorology of the atmosphere above 200 km.

4. Meteorological Measurements from Artificial Satellites

The meteorologist is chiefly interested in artificial satellites not as probes of their immediate environment but as platforms from which to observe the less tenuous atmosphere below them, or even the earth as a whole. The first major meteorological experiment to be mounted on a satellite is that carried on Explorer VII (1959ι) to determine the distribution over the earth of the net inward flux of solar radiation and the outward flux of terrestrial radiation. The aim is to locate the regions where sources or sinks of heat exist in the atmosphere, and to follow their changes in time. The link with the changing circulations which bring our weather is clear. Whether useful precision can be attained is not yet established.

A second experiment which has been proposed[4] is the continuous monitoring from a satellite of narrow regions in the infra-red emission bands, particularly of CO_2 and H_2O, which comprise the outgoing terrestrial radiation. This should allow computation of the pressure-temperature-height relation in the atmosphere below, at least in the upper and middle stratosphere. In a single day a satellite successfully instrumented in this way could transmit as much information as hundreds of rockets. Another experiment, which is at present being prepared by a group from the Meteorological Office for possible inclusion in the Scout satellite programme, aims at frequent determination of the vertical distribution of ozone in the atmosphere, by spectroscopic examination of the sunlight as the vehicle passes into and

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FIGURE 5. Cloud in the Caribbean region photographed from an Atlas rocket at a height of about 400 miles. In the centre foreground is the island of Hispaniola with well marked lines of cumulus clouds covering the coastal hills. The eastern part of Cuba can be seen, with heavy cloud cover. The extensive cloud patch south of Cuba, covering Jamaica, is associated with a weather disturbance of the type known as " easterly wave."

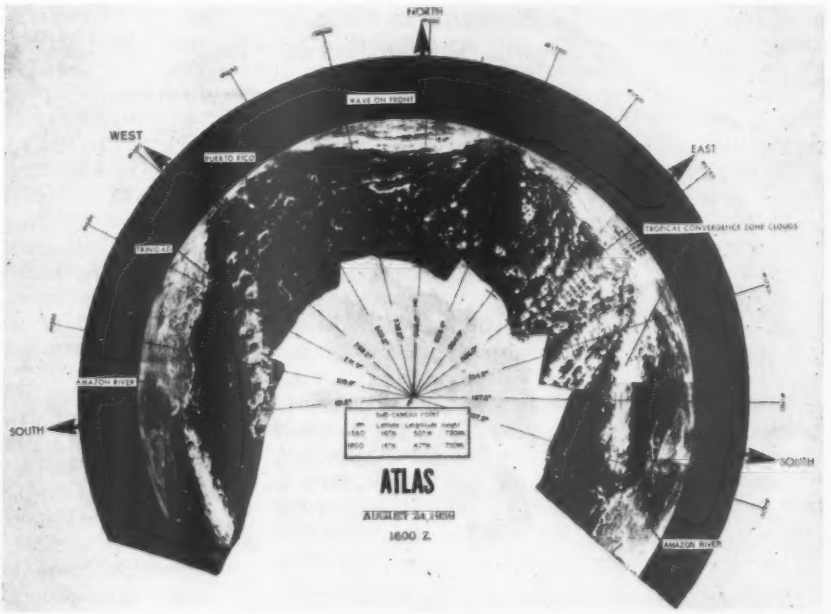

FIGURE 6. Composite photograph of an extensive region of the tropical and sub-tropical North Atlantic Ocean, taken from an Atlas rocket at a height of 730-750 miles. Cloud systems associated with a North Atlantic front in latitude 30°-35°, with a Caribbean easterly wave, and with the inter-tropical convergence zone in latitude 0°-10° N can be seen.

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out of the earth's shadow. The method is applicable in principle to other trace gases in the atmosphere.

5. Cloud Observations from Artificial Satellites

The meteorological aspect of the use of artificial satellites which has caused the greatest public interest, because of its immediate connection with weather forecasting, is the possibility of transmitting by television pictures of the cloud systems over wide areas of the earth where observation is at present inadequate. N.A.S.A. and the American Weather Bureau have planned an artificial satellite* which will transmit two 500-line pictures, one covering an area of about 800 miles diameter, the other in more detail an area of about 80 miles diameter. Thirty pictures will be observed in each revolution, the information being stored and transmitted on demand to the receiving station. The quality expected from the system can be judged from Figs. 5 and 6, which are not transmitted pictures, but are respectively a single frame and a composite picture from a film recovered from an Atlas rocket head which had a trajectory between Florida and the neighbourhood of Ascension Island. Information of this kind will be of less value in Western Europe than in countries surrounded by unfrequented oceans and subject to the compact and violent tropical cyclone. If a continuous television cloud watch is established, and this is the declared intention of N.A.S.A., it is reasonable to hope for at least a marginal improvement in weather forecasts in regions such as the British Isles which are well covered by conventional observation, and for the establishment of a reliable storm-warning system in tropical and unfrequented regions. We may also hope that examination of the large-scale cloud systems will give considerable help in our efforts to understand the general circulation of the atmosphere.

6. Meteorological Requirements in Vehicles

It will be seen from this summary of possibilities that the meteorologist's chief need in the field of rockets is for numerous ascents on a widely distributed network, and that most of his present interest lies in the region below 100 km. Cheapness and either reliable self-destruction in the air or very small dispersion are therefore essential. The most useful meteorological satellite would be on a circular orbit, nearly polar, at minimum distance compatible with reasonable life, with an axis stabilised in the direction of gravity. The return in understanding of the atmosphere would be great. The practical return in improvement of short-term weather forecasts, particularly of the hurricane-warning type, would not be negligible in any region of the world, and would be considerable in tropical regions, the oceans and the Southern hemisphere. Few meteorologists would wish to promise a substantial increase of forecasting accuracy in return for opportunities for upper atmosphere research, but if a break-through in the field of longer-range and seasonal forecasting is ever to be achieved satellite observations can be expected to speed it.

Acknowledgment

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Range Instrumentation Problems

BY

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1. Introduction

As this is a wide subject area with many specialised fields, a broad review from a systems viewpoint would appear to serve the purposes of this Symposium best. Also, such a review may help to show that the importance of range instrumentation in upper atmosphere research is rather more fundamental and widespread than generally appreciated.

The first problem is to determine the boundaries of the subject area. The lateral extent of the atmosphere is of course world wide. However, in the vertical direction its upper limit is steadily increasing as it is more intensely probed and as the importance of its diffuse structure becomes more apparent. Thus, at present, only planetary probes go beyond the domain of the upper atmosphere. Even some of these, such as solar probes, are intended to investigate phenomena that have secondary effects on upper atmosphere conditions. Hence, from the instru-

*"TIROS 1" successfully launched on 1st April 1960.

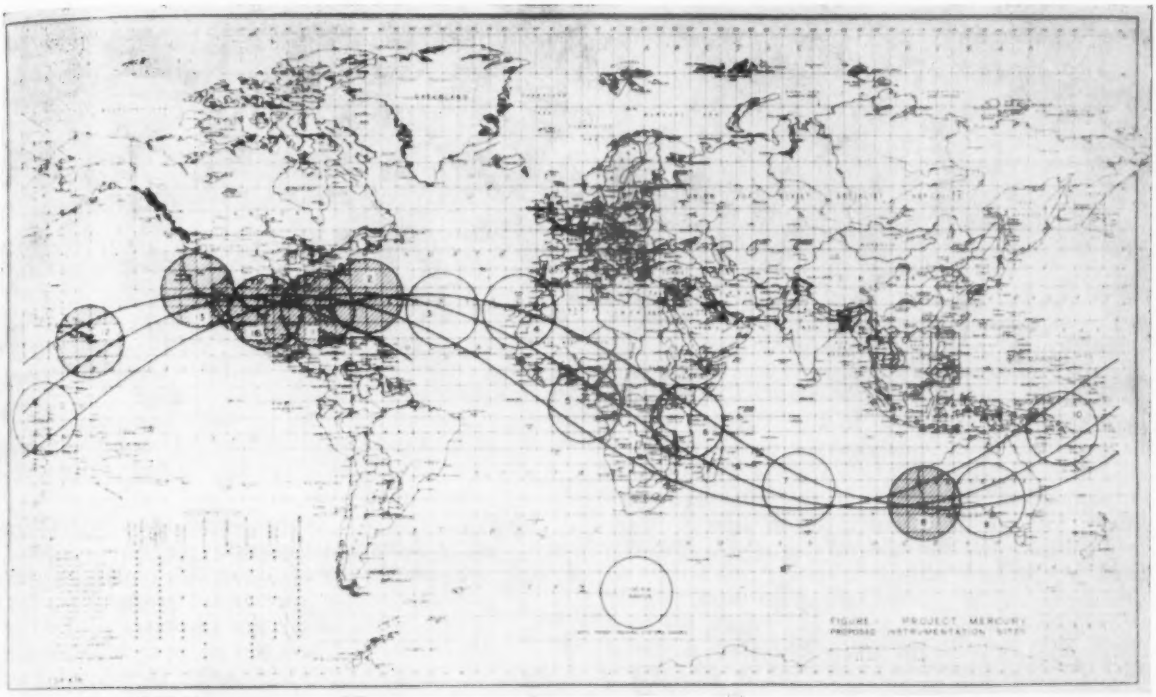

FIGURE 1. " Mercury " chain.

mentation viewpoint one must consider the problems associated with satellite probes orbiting at distances between several earth radii and minimum orbital heights in addition to the problems of balloon and vertical rocket probes.

Within these boundaries the most significant atmospheric region is below altitudes at 500 km. and within this domain the system instrumentation problems are those of deployment and overall resources and not primarily those of instrument technology as might be expected at first sight. It is remarkable indeed that during the past five years instrumentation technology has been so rapidly developed to exploit the capabilities of probes to such a degree that systems technique is not the primary problem of atmospheric research. Undoubtedly this state has been achieved mainly by the stimulus of military programmes and the I.G.Y. so that today one almost takes for granted the incredibly efficient miniaturisation of satellite-borne instruments at the one extreme and the massive complexity of the Jodrell Bank station at the other extreme. Certainly they are evidence that the technology exists of making and recording measurements by probes over most of the field of upper atmosphere research.

2. General problems

Broadly, the problems of the next five years in any expansion of upper atmosphere research are:

(*a*) to deploy the range instrumentation widely enough to give adequate coverage,

(*b*) to expand the associated data processing and computational facilities,

(*c*) to improve the overall performance of the various instrumentation systems as a whole, both from accuracy (or information bandwidth) and coverage aspects.

Considering the first question, it is a matter of fact for remote sites that the cost of site services, communications and personnel support can frequently outweigh the cost of the instrument. Thus even with considerable technical ingenuity in simplifying instrumentation and the maximum use of well developed station sites, only partial though essential savings will be achieved. A good example of such a simplification is the elegant photoelectric theodolite developed by Dr. Willmore of University College for the opitcal observation of satellites[1] to obtain the same data as the large Baker-Nunn camera. However, a single composite instrumentation centre of 500 km. range can only observe over 0·1 per cent of the earth's atmosphere below a height of 500 km., while this percentage is only increased to 2 or 3 per cent if there is no range limit.

Until recently the general concept of a range instrumentation system has been confined to the region covered by one, two or three such instrumentation zones; this has served adequately for the propulsion, free-flight and re-entry phases for rocket probes such as Skylark and Black Knight. However, with the advent of satellite probes the concept has been put forward by the U.S.A. of a world wide range including the partial integration of some existing range instrumentation zones on the Atlantic, Pacific and Australian ranges supplemented by additional linking stations. Even so, the whole Project Mercury chain, which is the most advanced, will only

reliably cover about 8 per cent of the atmosphere below the height of 500 km. (Fig. 1). This chain does not represent the total coverage of the atmospheric domain as there are at least two other global chains sponsored by the U.S.A. (as will be discussed later) and many major individual stations such as Jodrell Bank and Slough.

The second problem area of data handling and analysis is also a logistics problem although of a different kind. Currently the best general purpose digital computers and peripheral equipment are technically adequate to the task. But if any significant research programme is to be undertaken, their capacity will have to be expanded by parallel units or larger machines, despite any refinements in mathematical processes that may be developed.

The final problem area, the continued improvement of instrumentation performance, is an obvious need in any expanding programme. However, in this there is one aspect that needs some emphasis, namely that there is no natural division between what is called "range instrumentation" and what is called "probe instrumentation"; they are in fact part of the one system. In the initial planning it is most essential that an overall system design approach be adopted despite the fact that the installations on the ground and in the probe are the responsibilities of different authorities. Again the same attitude must be preserved in any subsequent modifications of the system as the difficulties in changing a widespread ground installation are much greater than the changes possible in probe transmissions. The only division line that should be drawn, if at all, is within the probe itself, for to some extent the transducers may be separately regarded from the data transmission system provided their outputs remain consistent with that system. However, the transmitters within the probe, their radiation power, modulation, aerial system, electrical supplies and environment control are all vital factors affecting the extent and quality of data received on the ground and hence the final results. Similar considerations also apply to optical tracking systems depending on the illumination from the probe.

To give these broad problem areas some perspective, it is as well to look at the kinds of range instrumentation systems that currently meet the needs of upper atmosphere research and their performance. From their merits and limitations one can begin to see which may be the most promising for future deployment and in what respects technical development would be rewarding. To do this it is convenient to divide range instrumentation into the following four fields:

(*a*) *Probe location* from both active and passive aspects; that is, active location for guidance and safety purposes, and passive location for co-ordinate determination in axes such as geoconcentric axes and Universal Time for fixing other observations.

(*b*) *Data recovery from the probe* generally by telemetry, possibly by recovery of tapes, photographic plates and samples or by telecommunications with an observer in the probe.

(*c*) *Observation of probe-induced phenomena* such as the observation of sodium clouds, "window", ionisation and wake effects and re-entry luminosity.

(*d*) *Measurement of the experimental environment*, i.e. the general observation from other instruments of phenomena such as the meteorological conditions, ionospheric state, magnetic field, cosmic ray and solar activity at the time of the probe measurements.

3. Probe Location

The "active aspect" of probe location is the presentation in actual time (so-called real time) of the probe's position for purposes such as guidance and control, safety or acquisition by other tracking instruments. This process involves the following steps:

(*a*) the measurement of the probe's position with respect to the observing instrument axes,

(*b*) the transmission of this information to some central computation point,

(*c*) the correlation of this information with that of other observing instruments,

(*d*) the computation of the probe's position in either geoconcentric or some other general co-ordinate system at the actual time of observation and frequently its predicted position at some future time,

(*e*) the display of this information to monitoring staff or the insertion of it into an automatic ground control system with transmission links to the probe,

(*f*) the determination of acquisition bearing values (and rates) for other instruments at different locations in that instrument's axes, and,

(*g*) the retransmission of this information to the respective stations.

In general, active systems are electronic throughout although in one or two special applications optical tracking, via servo telescopic systems, has been used to observe the motor luminosity during the propulsion phase in good night optical conditions. However, optical methods generally have been preserved for the more passive aspect of probe location. Before considering electronic systems in detail, it is as well to note that many instruments can be used in both an active and passive role. The main difference in the two roles is usually in the smoothing time and "curve fitting" procedures used with the data output if the latter is in an easily used form. Short smoothing times only are allowable for instruments when used in the active role to preserve as nearly instantaneous a measurement as practicable,

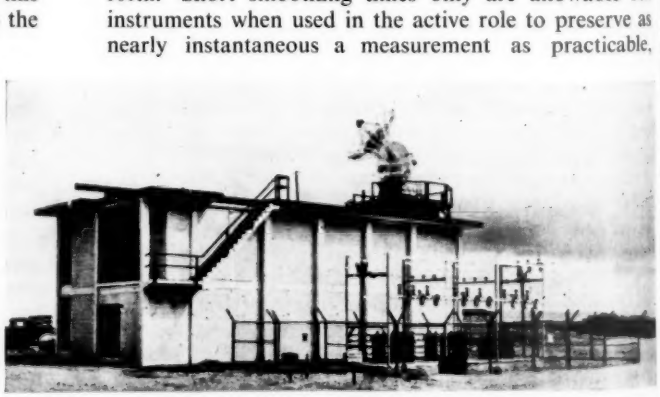

FIGURE 2. FPS. 16 radar.

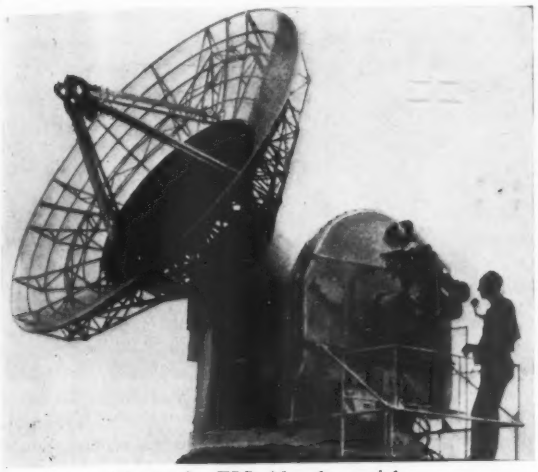

FIGURE 3. FPS. 16 radar aerial.

while in the computation, detailed calibration corrections may not be possible and approximate mathematical solutions may also have to be used to shorten the computational programme. These limitations do not apply in passive cases when the full accuracy intrinsic in the measurement may be extracted.

There are three basic forms of electronic instruments for probe locating, namely radar, interferometer and doppler, which may be used either singly or in combination. In the radar field the tracking instrument most widely used in the West is the American AN/FPS.16 radar, manufactured by the Radio Corporation of America. (Fig. 2). This is a precision instrument designed for working in conjunction with a probe-borne transponder and in these circumstances is capable of tracking ranges of some hundreds of kilometres. In its present form, acquisition information is required but improvements in its self-acquisition capabilities are being made. The normal peak power output of the transmitter is 1 MW. (but may be increased to 3 MW) with selectable pulse lengths and repetition frequencies. The 12 ft. diameter parabolical antenna (*see* Fig. 3) has a gain of about 45dB. This pedestal has been precision engineered and the accuracy of the radar is 0·1 milliradians in angle absolute (random errors being smaller). The accuracy in range is about 15 ft. absolute, again with smaller random errors. In both cases bias errors are removed by calibration on fixed beacons. This radar has wide application in launching and re-entry phases of most probes and has some use in satellite probe tracking for close earth orbits (*see* Fig. 1). Its main disadvantage is its high cost as an instrument and the weight of the transponder and its power supplies which must be carried in the probes.

Other tracking radars that are used in upper atmosphere balloon and rocket probe location are modified versions of the two war-time gunnery radars, the British No. 3 Mark 7 and the American SCR.584. Although less accurate and limited in range compared to the FPS.16, they nevertheless are still quite useful. A system using a modification of the No. 3 Mark 7 sets for direction finding on a 6 cm. beacon in the probe has been used for tracking missiles such as Skylark.

FIGURE 4. Minitrack aerials and van.

The interferometer has so far been the classical radio method of measuring satellite probe positions, in the form of the U.S. Minitrack system. This is the prime equipment used for transit observations of probes in relatively low altitude orbit for which the probe carries a miniaturised C.W. oscillator of very low power. A single station observation does not completely determine the probe's position but merely two cones, *i.e.* a transit time. However, by a series of observations and a process of successive approximations, precise orbital elements may be obtained by the method developed by Groves and Davies of University College, London[2]. Thus this technique is essentially a passive system although two adjacent stations simultaneously observing the probe could be operated as an active system. The original U.S. Minitrack chain comprised 12 stations set up for the I.G.Y. located between 35° North and South. This chain is being extended to cover more highly inclined orbits by four new sites in Newfoundland, Alaska, Central U.S.A. and England. Also, the existing stations are being improved. The frequency of operation will be changed from 108 Mc./sec. to 136·5 Mc./sec. The general system characteristics have been described in Ref. 3. The resolution of the system is 3·6 seconds of arc at 108 Mc./sec. although the overall accuracy is limited by uncertainties in ionospheric deviation to about 20 seconds of arc under quiet night-time conditions and to between 2-3 minutes of arc during the day. Under present arrangements high gain aerials give an effective beam width of 110° in the meridian plane by 10° East-West (Fig. 4). Improved angular accuracy is being sought by use of aerial spacings of 800 ft. compared with 500 ft. in the existing system. The layout of the aerial system is also more elaborate, permitting the choice of a North-South fence as at present and an East-West fence or a near hemispherical coverage. The data read-out system is also being improved to make it suitable for direct insertion into a teletype network so that very little processing is required by station personnel.

So far as the instrument itself is concerned, the main problem is in the initial and periodic calibration of the aerial field. This is a necessity with all interferometer

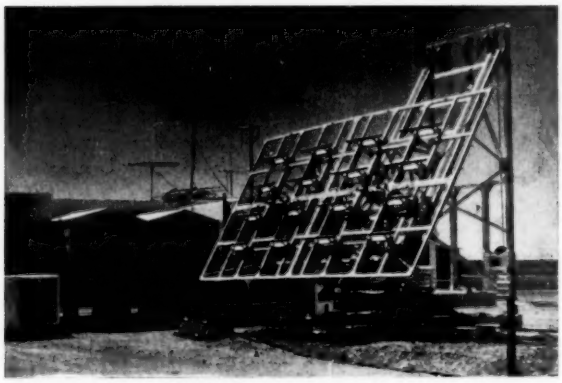

FIGURE 5. Doppler aerials.

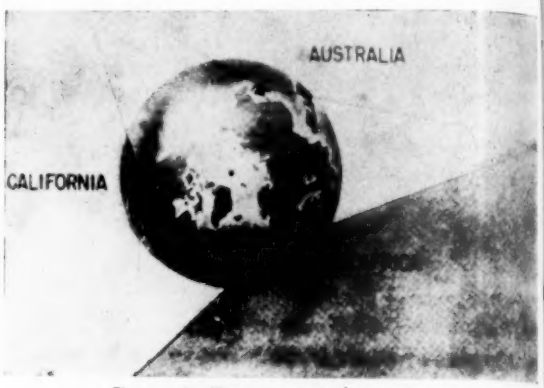

FIGURE 6. Deep space station coverage.

systems. The calibration involves high flying aircraft provided with both beacons and time correlated flashing lights for ground observation by ballistic cameras. In the case of Minitrack, this is much simplified by the location of one ballistic camera at the nominal centre of the aerial field. Otherwise, the equipment is reliable and easily maintained, but must be provided with a high grade teletype link to the control centre. As an overall system, N.A.S.A. have had to provide an extensive data processing centre for this individual station calibration determination for the correction of observations and the subsequent overall determination of orbital elements from the corrected observations of the chain of stations. A functionally similar interferometer equipment has also been developed in the U.K. by the Royal Aircraft Establishment for their own observation of U.S. satellites carrying a Minitrack beacon.

Interferometer systems have also been developed for probe position determination where the flight path is in the form of that of a vertical sounding rocket or ballistic missile. For Skylark, G.E.C. have developed an interferometer for determining the missile attitude from the ground for experiments where the probe attitude as well as its position is of importance. Another range system interferometer is based on the Cotar angle measuring equipment which uses a C.W. beacon in the probe, operating in the band from 100 to 500 Mc./sec. and should give angular accuracies of the order of 50 parts per million of direction cosine. The equipment is capable of local arrangement into an integral site system with its own display and computing facilities. Thus it is an active tracking system when so used with two or more sites simultaneously observing the probe.

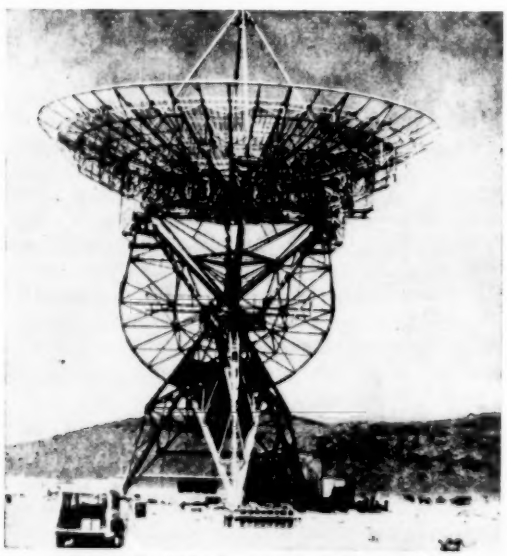

FIGURE 7. 85 ft. deep space tracking aerial.

Doppler systems have been in general use at ranges for missile instrumentation (*see* Fig. 5) and this technique is being adapted for space probe applications where the Kepler equations apply. In these circumstances it is possible to correlate theoretical velocity changes with those actually observed to determine an orbit with great precision. Multi-receiver trilateration arrays are not required as is the case in atmospheric guided flight where the precise equations of motion are not known. For the Kepler case, good data can be obtained with a stable oscillator in the probe in a one way doppler system as described by Guier[1] of Johns Hopkins University A.P.L., who directly determines the six orbital elements, the transmitter frequency and a refraction parameter from the doppler shift frequency by making full corrections throughout. The errors due to ionospheric refraction at frequencies below 400 Mc./sec. are at present the limiting factor but this can be reduced by a technique of transmitting two harmonically related frequencies above 100 Mc./sec. Frequency stability of one part in 10^8 has been achieved during flight, the total radiated power 200 MW.; the transmitter system weighs about 10 lb. and draws about 2 watts continuous power supply. An accuracy of about 8 km. has been achieved with a single station and on this basis it has been estimated that a global chain of about ten relatively simple receiving stations would give tracking accuracy about 1/5th km. in a 500 km. orbit. Such a system is a "passive" one.

Doppler ranging is used in the N.A.S.A. deep space tracking stations in conjunction with angular information obtained by normal auto-follow tracking techniques. Why the term "deep space" is used is apparent when one looks at the simple illustration shown in Fig. 6. Although

a very wide coverage is obtained from any one such site, it will be seen that continuous global cover cannot be achieved with the nearer orbits, but this is not required as these stations are intended to track well beyond the upper atmosphere domain. Continuous tracking to large distances from the earth necessitates the most sensitive receiving equipment practicable. Thus large tracking aerials are required; a typical example of this is seen in Fig. 7. This is a standard 85 ft. dish with sidereal mounting that N.A.S.A. are providing for their deep space network. The stations can operate independently, as does Jodrell Bank, but it is intended that a chain of about six such stations (including one at Woomera) will eventually operate as an integrated system. The antennae are more or less general purpose and can handle a large number of frequencies, being able to transmit or receive and thus will also be used for telemetry and command link purposes as well as tracking. The antennae will have auto-follow, programme tracking or manual tracking capabilities. The first such installation at Goldstone in California[5] is directly coupled in operations to two I.B.M. 704 computers in Los Angeles, one active and one stand-by, to smooth incoming data and to determine the probe's position and also to correct the programming of the antennae bearings. As remarked earlier, most of the foregoing active systems can be used for obtaining more precise probe position information subsequent to the time of observation by using better smoothing methods, more precise calibrations and more rigorous mathematical models. Of course if one has an I.B.M. 704 computer in circuit with the instrument as does the Californian station, presumably further refinement of the data and computation is not necessary. As these are vast and expensive installations designed for lunar and planetary probes they are not likely to have much direct application to upper atmosphere research beyond the probing of solar phenomena causing secondary atmospheric effects.

Optical methods of observation come into their own in the passive methods of probe location. These methods obtain their precision from:

(*a*) the established techniques of geometric optics,
(*b*) accurate calibration methods such as observations against star backgrounds,
(*c*) the general stability and ruggedness of installations, and
(*d*) the facility for laboratory measurement on relatively simple records made in the field.

Three optical and recording systems are generally used:

(*a*) fixed lenses and plates with a wide field of view as in the so-called ballistic camera, perhaps more accurately called photo-theodolite,
(*b*) narrow field of view tracking cinematograph cameras as used in kine-theodolites which track the probe and record its elevation and bearing as a theodolite, the film being used both to record these values and for collimation of the apparent probe displacement with respect to these pointings, *i.e.* as a bore site correction. Askania and Contraves are two such instruments,
(*c*) a combination of these two systems called the Baker-Nunn camera which arranges film transport so that the apparent position of the satellite is recorded with respect to the stellar background by double exposure, provision being made for stopping their relative velocities in the focal plane by alternatively tracking at satellite and sidereal rates. (Fig. 8).

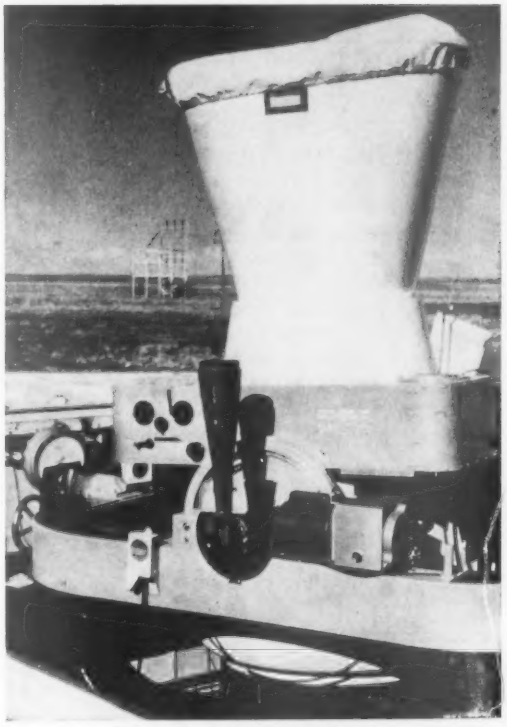

FIGURE 8. Baker-Nunn Camera.

Both the photo-theodolite and Baker-Nunn system collimation and lens calibration are determined by a star field observation programme. The kine-theodolites usually calibrate against ground markers as they are not capable of the high angular precision of the other two systems. All these systems require some assistance from the probe in providing appropriate illumination and contrast.

Kine-theodolites are quite effective at tracking probes at night during the propulsion phase when there is sufficient illumination for both the operator to track and the system to record suitable images, but they become ineffective at higher altitudes unless a large continuous light source can be provided. This problem does not arise during re-entry when a considerable amount of light may be emitted by energy dissipated into the atmosphere as will be seen later. Film records from kine-theodolites are normally read in a special viewing machine which permits the bore sight and timing information to be incorporated with the altitude and azimuth readings and records the result on tape. This is fed to the computer for a least squares triangulation solution. Contraves kinetheodolites have a basic accuracy of approximately 10 seconds of arc, and depending on layout of stations and height of probe can give positions to about 100 m.

FIGURE 9. Ballistic camera record, Skylark flashes.

For satellite observation, even with unfavourable base line lengths and provided an image is recorded, the accuracy obtained would be about 1 km. (*see* Refs. 6 and 7). In view of general illumination problems and cloud obscuration, this technique is not much used for satellites.

Ballistic cameras[8] with a wide field of view are fixed and not tracking instruments, and can be pre-orientated to cover the probe's trajectory. They do not need a continuous light source; in fact, if one does exist, as for example during propulsion and re-entry phases, arrangements must be made to time chop the image by special shutters to provide discrete reading points. The more usual technique is to emit flashes, either electronic or pyrotechnic, from the probe, of considerable brightness to act as a point source of observation against the stellar background. This enables shutters to be left open for a period covering several observations without undue blurring or elongation of the star trails. Flash detectors (basically photomultipliers) are also used in conjunction with ballistic cameras to obtain precise time correlation of the flashes. The range system uses Wild BC4 cameras which have field of view of 46° × 46° with 210 mm.f/4·2 Aviotar lenses or 33° × 33° with 300 mm.f/2·5 Astrotar lenses. These are laid out in a siting configuration to achieve maximum accuracy for the predicted position of the flashes. The star field exposure may be the same as that used to record the flashes or may be a separate exposure made immediately after (Fig. 9). The times of starting and terminating the exposure are accurately recorded in Universal Time.

The plate co-ordinates of the ends of suitable star trails are read in a machine such as a Zeiss comparator which can determine plate co-ordinates to 0·1μ and likewise the flash positions; thus from star tables and station latitude and longitude, accurate altitude and azimuth angles may be computed. The subsequent triangulation computation can be carried out in the same way as for kine-theodolites. The angular accuracy of the BC4 cameras, when all steps are taken to reduce errors due to emulsion distortion, inaccuracies in determining true centres of images and atmospheric distortions, is about 3 to 4 seconds of arc. With suitable site geometry and slant ranges of about 500 km. an array of these cameras and flash detectors gives positional accuracies of 2 to 4 metres and velocity accuracy of about 0·5 metres per second under Keplerian conditions. This then represents one of the most accurate instrumentation systems available without going to the full complexity of a Baker-Nunn camera. However, the ballistic camera technique is not particularly applicable to satellite work as it requires high energy flash sources in the probe.

The Baker-Nunn camera has been developed to meet this satellite observation problem as it observes during a favourable period when the atmosphere is in the earth's shadow but the satellite is illuminated by sunlight[9]. With a highly reflective surface the satellite's image is then bright enough to be recorded against a stellar background.

The lens of the camera is a modified Schmidt type of design. The field of view is 30° × 5° and the optical design is such that 80 per cent of light reflected from the satellite is condensed into a 20 micron circle of confusion at all points in a 30 degree field. 55 mm. Cinemascope film is used as the recording medium, each frame being in the form of a strip about one foot long tensioned around the cylindrical focal plane. An automatic film transport system permits the taking of successive exposures at intervals from 2 secs. to 32 secs. apart.

In operation, the camera tracks a close satellite with an accuracy of one per cent, thereby increasing the effective exposure time, as compared to a trailed image, by a factor of 100. By this means under the most difficult circumstances, when the satellite is at apogee, its brightness will be about three star magnitudes above the film sensitivity limit.

To achieve the desired 2 seconds of arc angular accuracy, it is necessary to measure satellite image positions relative to star images on the film. Stars will

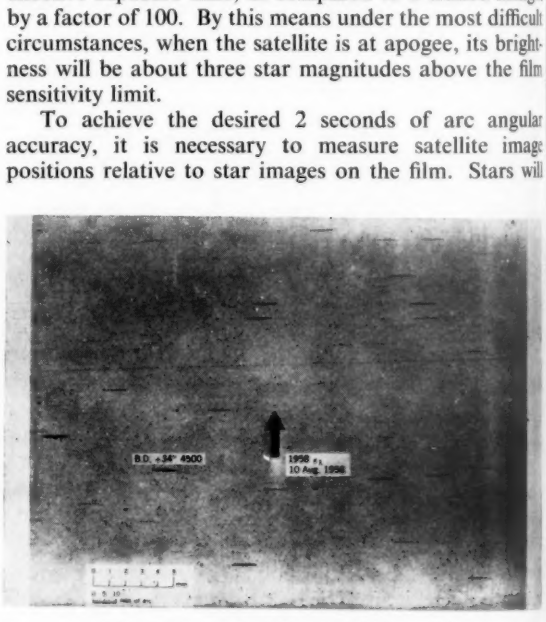

FIGURE 10. Baker-Nunn satellite record.

trail if the camera is operated at satellite rate and only those stars brighter than magnitude 5·8 will record. On the average two or three stars of this brightness fall in the 5-by-30 degree field. By arranging sharp breaks in the trails, reference points for measurement and time determination is obtained. But there is the problem of film stretch and distortion over the area of the film and to overcome this a background of faint stars is provided by making the camera track at sidereal rate for a fraction of each cycle. (Fig. 10). The camera is on a tri-axial mount to permit tracking in any direction across the sky. The altazimuth axes are clamped in a fixed position during tracking, which is accomplished by swinging the camera body about the third axis.

The angular accuracy achieved is limited by timing as well as image measuring accuracy. At the greatest angular velocity with a close satellite, a measuring accuracy of two seconds of arc corresponds to a time determination of 1/2500 sec. of time in the extreme case. It is impossible to achieve this in the field but an accuracy of a few milliseconds with respect to Universal Time can be obtained. The angular accuracy achieved is 1–2 secs. of arc, and the limit recording is approximately 13 stellar magnitudes at close satellite angular rates. At Woomera the Baker-Nunn camera photographed Vanguard I, the so-called "grape-fruit" satellite, at a distance of 2500 miles, and the Explorer VI, the paddle wheel satellite, at a distance of 14,500 miles, both of these observations being the most distant achieved anywhere. At present the cameras are deployed on a global basis and direct triangulation is not possible, but the observations are used to determine orbital elements in the same manner as the Minitrack observations.

4. Data recovery from probes

Although no great distinction should be made between probe and range instrumentation, I do not propose dealing with the former. A comprehensive review of satellite transducers and environment control is given in Refs. 10 and 11.

For Skylark the telemetry system currently used is that known as the 465 Mc./sec. 24—channel system. In essence it comprises an airborne transmitter of a nominal carrier frequency of 465 Mc./sec. which is pulse amplitude modulated at a nominal repetition frequency of 145 KCS ±10 per cent, *i.e.* this sub-carrier is frequency modulated ±10 per cent. This frequency modulation of the sub-carrier oscillator is determined in the oscillator circuit by either inductance or voltage transducers connected by a commutator switch of 24 segments and rotational speeds of 40, 80 and 120 revs./sec. to time multiplex the transducer channels.

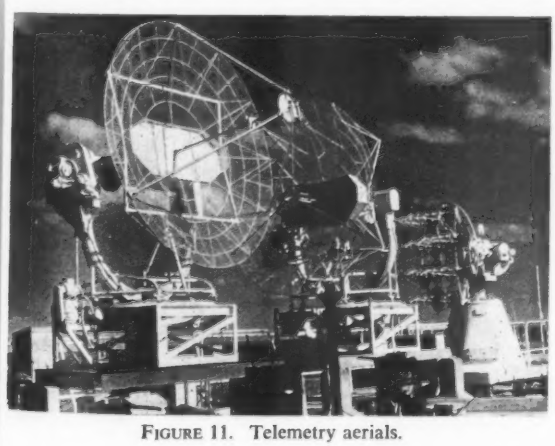

FIGURE 11. Telemetry aerials.

The ground equipment consists of tracking auto-follow aerials (Fig. 11), receiver and detector which recover the original sub-carrier, and a frequency demodulator followed by an electronic switch which is capable of synchronising with the sender switch. Synchronisation is carried out by generating a strobe for each channel at the correct phase in the switch channel. These strobes can be connected to give brightening pulses, as required, to a cathode ray photographic recording tube.

For later firings of Skylark at Woomera, it is intended that the SRDE broadband telemetry system be used. This system is a development of the earlier system but is a broadband system. The final stage of time multiplexing produces 8 channels having a bandwidth 0 to 3·5 Kc./sec. which are wide enough for measuring flight vibration characteristics or other transient phenomena. Alternatively each of these channels may be sub-multiplexed into 24 low speed channels by a mechanical commutator giving 200 samples per second. There are several transmitter schemes that may be used. The ground equipment is extensive and highly sophisticated.

For any British satellite probes it would appear that consideration will have to be given to the use of American telemetry systems on the assumption that some time sharing of U.S. tracking networks can be agreed. I presume this is already in train for the Scout programme. At Woomera facilities already exist in the 108 Mc./sec. band (including transmitters for interrogating U.S. satellites fitted with tape storage systems) and also in the 20 Mc./sec. band for both Russian and U.S. satellites.

Data can also be recovered from probes by internal recording or sampling with subsequent physical recovery of the probe after it has returned to earth. Rugged miniature tape recorders have been developed for storing telemetry type signals and successful firings have been carried out at Woomera. An extensive organisation exists for locating and recovering such records and rocket components. Special cross country vehicles have been developed for this task. Probe film records are also a valuable method of recording data and direct observation of cosmic radiation or general phenomena. The classic example is the film record obtained in an Atlas firing last year showing a complete meteorological system over the central Atlantic[12]. Recovery of plates will also be of great value in astronomical observations, while samples of the air and biological samples exposed to upper atmospheric conditions can also be recovered.

5. Observation of probe induced phenomena

Skylark and other rockets have used grenades ejected at altitude to obtain information on the wind and temperature structure of the atmosphere from the propagation of the sound to microphones deployed on the ground. The flash of the grenade explosion is recorded

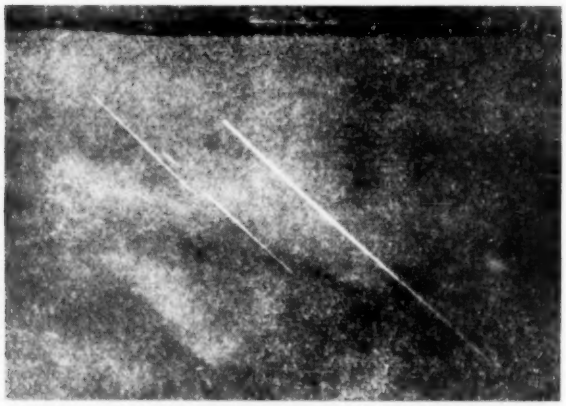

FIGURE 12. Jupiter re-entry photograph by ballistic camera.

on ballistic cameras as already described, from which the position of the sound with respect to the microphones can be determined. Simple hot wire microphones (as formerly used in artillery practice) are laid out in an interferometer array so that the precise time and direction of the wave front can be determined. The microphone consists of an open vessel across which platinum wires are stretched and heated by an electric current. The arriving sound pulse momentarily cools the wire and changes its electrical resistance, hence the change in voltage can be recorded. Timing accuracies of about one millisecond are required. Using all known meteorologic information and redundancy of information from a seri of vertical grenade explosions and a number of micro phone arrays, the vertical atmospheric structure can b obtained by a series of approximate solutions[13].

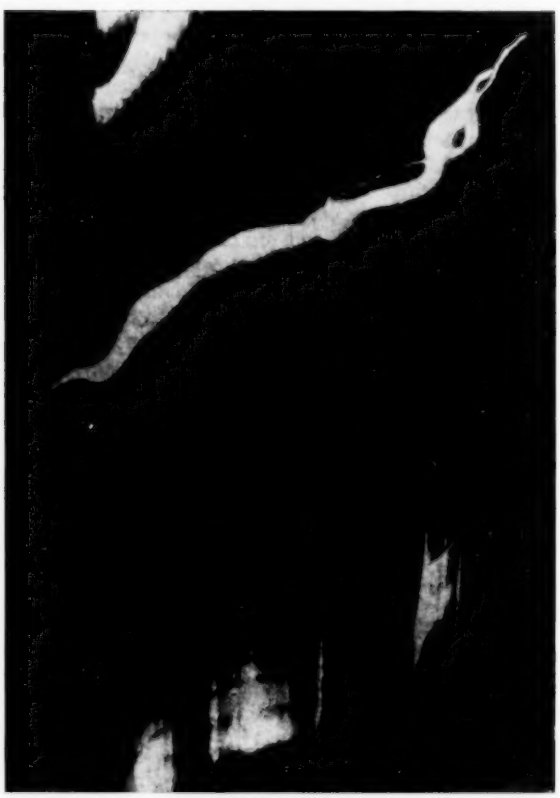

FIGURE 13. Jupiter re-entry ballistic camera spectrograph (ship-borne).

A somewhat allied disturbance to the atmospher that may be observed is the light (and sound) energ emitted from a probe re-entering the atmosphere a high velocity. Fig. 12 shows a photograph of a Jupite rocket re-entry on the Atlantic range—the several trail are different portions of the rocket carcass burning up This photograph was taken by ballistic cameras locate on the island of Antigua, B.W.I.[14]. Fig. 13 shows spectrograph again taken with ballistic cameras throug a 300 line/mm. grating and a two blade 100 r.p.m. chopper for timing. The installation was ship mounted hence the waviness in the traces due to its roll. The first order spectrum is from the Jupiter body. It show both emissions and absorption lines characteristic o aluminium oxide and sodium and atmospheric absorp tion due to water vapour. In this observation the empha sis is rather more on the effects of the atmosphere on th probe but obviously also gives some indication of atmo spheric structure. Similar observations with radiometer and photometers have been made of the energy in a number of spectral bands as defined by filter or receive cut-offs, from the infra red to the ultra violet. High speed tracking cameras may also be used to obtain general records showing the development of the radiation and its variability.

Other phenomena that may be introduced into th upper atmosphere by the passage of probes are th disturbance of the ionosphere and the blanketing o transmission to and from probes by the ionisation tha is apparent from the illustrations during re-entry.

Even more active disturbances may be caused by th ejection of sodium or "window" for tracking by radar

6. Experimental environment

As is normal, extensive ground observations mus be taken at the time of the probe to ensure as far a possible that the environment is defined. Normal meteorological observations are made both in the ex and re-entry zones for rocket probes or adjacent to th observation station when some special phenomena are t be observed. In the launching of Skylark ground wind are an important factor in determining the probe dis persion and use of simultaneous firings of smoke rocket has been made so that subsequent analysis of the trail may help to indicate the wind state (Figs. 15 and 16).

Other equipments provided are vertical incidence ionospheric sounders for determining $P'f$ curves. These data are required both synoptically and at high sweep rates of frequency immediately before and during probes

Another equipment at Woomera is a proton precision magnetometer. This automatically record values for the earth's magnetic field vector at the surface every three minutes and may be correlated with measurements made in probes to determine whether there are extra-terrestrial ring currents during magnetic storms.

Solar, meteor and cosmic ray activity are also o importance but so far there has been no requirement fo

FIGURE 15 (*left*). Wind finding rocket.

FIGURE 16 (*right*). Wind finding rocket—later.

such observations on the range as information from the general observatories has sufficed.

7. Conclusion

The wide extent of range instrumentation systems and deployment has been reviewed. It is apparent that the technology is adequate for current upper atmospheric probe programmes and that the main problems are those of logistics in deployment and data processing.

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Future Possibilities

BY

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MR. HAZELL HAS outlined some of the future improvements both in performance and in experimental facilities—such as attitude measurement or control—which it is hoped to be able to offer for Upper Atmosphere Research in the sounding rocket Skylark. I propose to try to look further ahead and, in particular, into the future possibilities in the United Kingdom of upper atmosphere and space research in earth satellites.

But before doing this I should like to rehearse briefly some of the arguments which suggest that sounding rockets and earth satellites will have a complementary part to play in upper atmosphere and space research in the future. In particular I shall try to answer the question "Why can it not all be done with sounding rockets?"

The near earth satellite has sometimes been described as a "long playing" sounding rocket. Just how "long playing" depends, of course, on the orbital altitude and, in particular, on the altitude at perigee. For a circular orbit this altitude is about 150 miles for an estimated life time of a week, 190 miles for a month and 260 miles for a lifetime of a year. For research at the lower altitudes, economics will tend to favour the choice of many sounding rockets rather than a single satellite, but, provided the satellite can take full advantage of its long life for observational purposes, it will be economically preferable at high altitude. The satellite also offers the ability, for a single launching, to sample conditions at a range of altitudes and, if desired, at all geographical locations.

Even with a life of only a week one satellite is potentially the equivalent of many thousands of sounding rockets.

There are some important areas of space research, such as, for example, ultra-violet astronomy from outside the earth's atmosphere, for which the instrumental payload would be very large, complex and costly. For this sort of work it would patently be absurd to throw the payload away after a few minutes use in a sounding rocket, and the long life satellite would obviously be preferable. This raises a general problem in upper atmosphere research by satellite, that, once launched there is little possibility of changing the nature of the experiment, and it would obviously be valuable, though difficult, to have considerable experimental flexibility in the satellite, and to be able to initiate changes and adjustments by command from the ground. The right overall approach is probably to use sounding rockets initially to give rough information and to indicate likely orders of magnitude of effects which it is desired to observe. These should provide adequate knowledge on which to design satellite-worthy experimental equipment. Satellite observations may then suggest further rocket firings before the firing of a further satellite and so on—so that sounding rocket and satellite would play essentially complementary roles.

I will mention one of, no doubt, many other respects in which satellites because of their long life, differ appreciably from sounding rockets. This is of significance in experiments which are concerned with measuring the properties of the tenuous atmospheric environment at very high altitudes. The sounding rocket is liable seriously to change this environment because of outgassing and of residual rocket exhaust products; the satellite, on the other hand, is sufficiently long lived for such effects to be reduced to negligible importance. It is probable that effects of this sort account for the discrepancy between atmospheric density at high altitude as deduced from sounding rockets and from the behaviour of earth satellites.

Let me now turn to the future possibilities of United Kingdom upper atmosphere or space research using earth satellites. As is well known, plans have been made for the launching of United Kingdom scientific experiments by means of the American satellite launching vehicle Scout. In addition our own satellite design studies have been authorised by the Prime Minister and are in progress at the Royal Aircraft Establishment.

I propose to deal only briefly with the former, and Mr. M. O. Robins, who is present today and who is intimately associated with the Scout project, will no doubt be prepared to say rather more about it.

The Scout launching vehicle, which has four solid rocket stages, is about 70–80 ft. long and weighs about 35,000 lb. It is to be launched by the U.S.A. from American sites and is capable of putting a satellite of about 150 lb. weight into a near circular orbit at about 300 miles altitude. The satellite will be spinning, with the angular velocity required to spin stabilise the final rocket stage, and no great precision is being sought in the achievement of a prescribed orbit. The payload capacity of Scout is too small to contemplate such complex projects as space stabilisation of a satellite for astronomical observations.

The launching of three Scout satellites has been agreed with the U.S. authorities, and the University experiments to be carried in the first Scout—to be launched towards the end of next year—have been agreed.

The R.A.E. satellite design studies are concerned with two aspects. Those are:

(*i*) the problems of adapting British military rockets either in existence or under development for the launching of satellites and space probes.

(*ii*) the problems of creating a satellite environment in which scientific experiments and sensing instruments can function satisfactorily.

The second aspect implies far more than the design of a suitable satellite structure. It includes studies of methods of providing long term power supplies, of means for stabilising and controlling the satellite attitude in space, or of providing continuous information about satellite attitude (for an unstabilised satellite), of means of providing long distance communication from the satellite to the earth for data recovery, and possibly from ground to satellite for command purposes. It also includes the problem of satellite heat balance, that is of maintaining within the satellite tolerable ambient temperature conditions.

The purpose of the studies both of the launching vehicles and the satellite environment is to expose and as far as possible to solve the many problems which arise, to get a better idea of the technical and performance capabilities at our disposal, and to provide a sound basis for the estimation of the development costs of a possible United Kingdom satellite programme.

So far as the satellite launching vehicles are concerned our aim has been to minimise the likely development costs by designing to make the greatest possible use of existing or proposed hardware and facilities. Thus we assume that Blue Streak, stripped of its essentially military trappings, but otherwise with the absolute minimum of modification would form the first stage booster. We have sought to evolve a launching vehicle which, again with the minimum of modification, would permit the launching of earth satellites or space probes ranging from a near earth near circular orbit satellite, through satellites or space probes on more eccentric orbits going out to an apogee distance of two or three earth radii, to space probes on highly eccentric orbits going out to the order of 100,000 miles or more from the earth. The near earth satellite requires a projection velocity of about 25,000 ft./sec. while the far space probe demands about 35,000 ft./sec.

The obvious vehicle to consider for the second stage is the existing rocket Black Knight. However, because of the rather large and heavy satellite payload which the Blue Streak/Black Knight combination proves capable of putting into a near earth orbit it appears desirable, while retaining the existing Black Knight propulsion system complete, to replace the present long thin tank layout, by a shorter and fatter design.

We have chosen as our aim for satellite life-time in all cases a period of one year. This means that perigee for all of the orbits must be about 300 miles. Now the powered climb for an earth satellite has an important difference from that of a long range ballistic missile.

For the ballistic missile the precise height at motor cut-off does not matter so long as the magnitude and direction of the velocity at this point is so chosen that the ballistic path passes through the target. In the case of the earth satellite the height at motor cut-off is of prime importance. This must be at least as high as the desired perigee, and the magnitude and direction of the velocity at cut-off then determines the precise height of perigee and the eccentricity of the orbit. For the average long range ballistic missile motor cut-off usually occurs at an altitude of between 50 and 100 miles; for our satellite some velocity has to be added at an altitude of at least 300 miles.

The usual approach to the powered climb pattern for satellite launching has been to complete one or two stages of rocket burning, and, having suitably adjusted the cut-off velocity at this point, to allow the last stage or stages and the satellite to coast up to the apex of its trajectory at the desired orbital height, and then to add the necessary velocity, by firing the last stage or stages, to inject the satellite into orbit. This could be done with the Blue Streak/Black Knight combination by closing down the Black Knight engine after its first firing with some propellants remaining, and allowing it to coast up to orbital altitude, then relighting the Black Knight engine and burning the remainder of its propellants to inject the satellite (and the empty Black Knight) into orbit. This would, in principle, allow a substantial payload to be put into a near earth orbit, but an additional rocket stage (or stages) is required for the space probes. This raises problems in controlling the attitude of Black Knight before "relight," and in relighting after a period of weightlessness, when the rocket propellants may be dispersed anywhere within the tanks.

These difficulties have been avoided and a number of other advantages been obtained by a novel approach, which, so far as is known, has not been advocated, and certainly not exploited, for satellite launching in the U.S.A. This is to use a "low-thrust" vernier stage, lit immediately after Black Knight propellant exhaustion, (and in our case using the same propellants—HTP and kerosine—as Black Knight) and operating throughout what would otherwise be the coasting period, until the appropriate orbital height and speed are reached. The initial velocity at Black Knight cut-off would be so arranged that the final stage would climb under gravity to the necessary orbital height, and the vernier motor would thrust always along the local horizontal, so that all its energy would be expended in accelerating the satellite, and none in doing work against gravity or drag, (since at these altitudes this will be negligible).

By this means the problems of liquid motor light-up after a weightless period are avoided, since the vernier motor would be lit before the termination of Black Knight thrust. This approach also greatly eases the guidance problem of ensuring the correct cut-off conditions, in that the low terminal acceleration near cut-off allows accurate velocity measurement, using long smoothing times by means of a ground radar sited roughly below the cut-off point.

The guidance accuracy requirements are difficult to pin down but they are clearly much less stringent than those for a long range ballistic missile. For a 2,500 mile missile an accuracy of one mile at the target demands an accuracy in cut-off velocity of 3 ft./sec. A "nominal" circular orbit at 300 miles altitude which oscillates between 290 and 310 miles only demands an accuracy at cut-off of 70 ft./sec. Adequate guidance accuracy can probably be achieved with only an auto-pilot, a radar transponder and a command link receiver in the final rocket stage, associated with a ground system consisting of a velocity measuring radar, a simple computer and a command link transmitter.

Extensive performance calculations have been made of launching vehicles following the general lines described, and these studies suggest the following capabilities:

(*i*) To put a satellite weighing about 1750 lb. into a circular orbit at 300 miles altitude. This would be accompanied in orbit by the empty third rocket stage weighing about 500 lb.

(*ii*) To put into an eccentric orbit with perigee at 300 miles and apogee at about 8,000 miles, a satellite weighing 400–500 lb.

(*iii*) To put into a highly eccentric orbit with the same perigee, but apogee at 100,000 miles or more a satellite weighing about 200 lb. Both these last two would also, of course, be accompanied by the empty third stage rocket.

The launching vehicles for these three tasks would only differ in the third vernier stage, and the differences here would be primarily in the propellant capacity of the third stage tanks.

The rather large satellite payload capacity in a near earth orbit immediately opens up the possibility of providing a stabilised platform for the carriage of a telescope of quite substantial aperture for ultra-violet astronomy, and it is on the provision of the necessary working conditions for this on which most of the satellite environment studies made so far have been based. As well as raising the problems of satellite stabilisation and control, this application raises all the other problems of satellite environment, many of them in an accentuated fashion. This work is being done in collaboration with the Working Group for Astronomy of the British National Committee on Space Research.

In addition it is proposed to study the environmental problems of two other satellites. These are:

(*i*) An unstabilised instrumentation package of perhaps 500 lb. weight capable of going out to a distance of 8,000 to 12,000 miles. The primary purpose of such a satellite would be to investigate the composition of the earth's atmosphere and its radiation and electro-magnetic fields.

(*ii*) A smaller unstabilised instrumentation package of weight about 200 lb., primarily to investigate the properties of the sun's atmosphere.

Generalised studies relating to "multi-purpose" satellites are not really feasible, and it is essential to be clear at the start about the properties of the equipment which the satellite is designed to carry, and its requirements in such terms as size and weight of equipment, the need for attitude information and the accuracy required, orbital requirements in altitude and orientation, power supply requirements, telemetry bandwidths, the

need for ground commands to the satellite, data storage and so on. On the two unstabilised satellites, therefore, we are, in collaboration with the appropriate sub-committees of the British National Committee on Space Research, sorting out typical compatible groups of experiments. We shall then study the problems of the satellite environment which needs to be built round them.

Perhaps the most interesting of these problems is that of satellite attitude stabilisation and control. The majority of satellites so far launched have either been allowed to tumble freely or have been to some extent spin stabilised by the rotation, imparted for accuracy purposes, to the final rocket stage. Long term stabilisation can only be accomplished by a control system operating in relation to a sensing system defining a system of axes.

Different systems of axes may be required for different purposes, and the particular system may have a marked effect both on the satellite design and on the control system. For example a satellite stabilised in space axes should have its three principal moments of inertia equal in order to minimise the disturbing torques due to the differential effect of the gravitational field of the earth. If the satellite is to be stabilised in earth axes, these torques may be used to provide restoring forces—then it may be advantageous to have one principal moment of inertia very small compared with the other two.

The problem of the disturbing torques is itself a complex one. The disturbing effects of the earth's gravitational field, and forces due to the sun's radiation pressures are calculable, but estimates of the effects on a satellite due to passage through the earth's magnetic field and through ionised regions of the atmosphere are less easily obtained. The effects of the impact of micro meteorites is also difficult to take into account because of lack of knowledge of their occurrence and magnitudes.

Once we have specified the disturbing torques and the required motion of movable equipment, such as trainable telescopes, in the satellite then the control system can be designed. Control torques can be produced by means of gas jet reaction systems, or by systems depending on the principle of the conservation of angular momentum, such as precessed constant angular velocity or accelerating flywheels. There is no general answer to the preferred system, in some cases a combination may be best.

The obvious choice for a reference system for stabilisation in space axes is to use the stars. For stabilisation in earth axes the use of the earth's magnetic field, of the direction of gravity or of infra-red horizon seeking devices are all possibilities.

The aim of a useful satellite life-time of a year coupled with the very considerable power requirements, not only of the experimental equipment and for communications, but also of the reference and control system, raises difficult problems in the power supply field. The total power requirements for an astronomical satellite for example, are likely to be large—of the order of 100 watts. In theory this can be obtained from less than a square metre of solar cells, but this assumes that the square metre is in sunlight, and normal to the incident light, and also that the cells are maintained at a reasonably low surface temperature in order to keep up their efficiency.

In practice the design of a power supply system raises a whole range of problems: the provision of the necessary solar cell area appropriately orientated to the sun whatever the satellite attitude, the provision of backing up chemical batteries charged by the solar cells which will carry the load when the satellite is in darkness, the isolation of cells which are not illuminated, the dissipation of unwanted heat both from the sun and internally generated and so on.

Telemetry from the near earth satellite presents no great technical problems, although more extensive ground receiver coverage than is now available would be an advantage. The communication problems obviously become more acute for the far space probe, but there are a number of ideas worth investigating which would ease this problem. There is a possible weight compromise between doing the initial processing of the raw observations (such as, for example determining the r.m.s. value of an observed quantity) in the satellite in order to reduce the communication band width required, and providing sufficient telemetry band width and power to enable the raw observations to be transmitted to the ground. There is also the possibility of adjusting the communication band width and information data rate continuously with range, so that the best possible use is made of the available power and time. This was, I believe, done on the Russian Lunik III.

Finally there is the possibility of the recovery of scientific instruments and observations, on photographic film, for example, from earth satellites. The problems of protecting an instrumented package against heating during re-entry are not particularly severe. Probably the most difficult, although again far from insuperable, problem is to ensure recovery into a specific area. This would demand an attitude control and reference system in the satellite to fire a retro-rocket, attached to the package, in the appropriate direction to initiate re-entry.

Summary of Discussion

BY

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SKYLARK AND SCIENTIFIC INFORMATION

A speaker from R.P.E. Westcott, remarked that he was glad to see that when vehicles were provided for instrumentation, good use was made of them.

The Raven motor used in Skylark was not a high performance motor. It was simple, rugged, did not need much design effort, had a short development time and was relatively inexpensive.

It was formed from wrapped and welded steel tube and had forged and machined end rings, heat treated to 80 tons/in.^{2}. The nozzle was of steel with a carbon insert. The motor had an asbestos phenolic expansion cone to

keep the weight down and the charge was a relatively cool plastic propellant of simple star section design. The motor had been developed in about a year by Bristol Aircraft Ltd. The cost of the Raven was of the order of £3,000.

The first firing lasted for 20 seconds, being terminated by a failure at the nozzle end. The second and third firings had been successful. Subsequently, some difficulties had arisen when a high pressure peak occurred on ignition in about one in five firings. This fault, however, had only resulted in two failures on the launcher and was now cured.

Although reference had been made to only one Raven motor, there were several variants including one having an increased performance; no vehicles had been fired with the motor of increased performance. As well as increasing the performance of the Raven motor by increasing the total impulse it was possible to fit a boost motor, the Cuckoo (so called because it kicked the Raven out of the nest!) having a total impulse of 81,000 lb.sec., about one quarter of that of the early Ravens; this was delivered in about 4 seconds. Cost of the Cuckoo was £700-£800.

A speaker from R.A.E. Farnborough outlined the capabilities of a two stage vehicle using Skylark as a first stage. He thought that such a vehicle could do better than Nike-Cajun which was able to raise some tens of pounds of payload to a height of 100 miles.

Mr. Hazell had mentioned a head weight of 250 lb. By using a Skylark as a high speed launching platform having the same all-burnt altitude as at present, about 80,000 ft., it should be possible to increase this figure of 250 to 600. By assuming that this 600 lb. included about 15 lb. of instrumentation and 5 lb. of telemetry equipment making up a total, including the flare and so on, of some 60 lb., a weight of 540 lb. could be assigned to the second stage motor. Mr. Hazell's figures had shown a mass ratio in excess of 0·8 for Raven; for the smaller motor under consideration a figure of 0·75 would be assumed leading to a weight of some 400 lb. for propellant.

The second stage could thus be summarised:

All up weight	600 lb.
All burnt weight	200 lb.
Propellant	400 lb.
Case	140 lb.
Structure and payload	60 lb.

These figures gave an overall mass ratio for the vehicle of 3. The propellant would be assumed to be fairly fast burning, having a burning time of 3 seconds and a specific impulse of 220 seconds, somewhat better than the figure for the Raven of 177 seconds.

To obtain an approximate figure for the second stage maximum velocity one must first estimate the first stage velocity with the extra second stage weight. Using the equation

$$V = Ig \log_e (m_o/m) - gt$$

one obtained an approximate first stage velocity of 4,470 ft./sec. but this must be corrected for drag. Drag could be estimated from the known fact that the Skylark normally reached 100 miles, implying a maximum velocity in vacuo of 5,350 ft./sec., whereas the equation above implied a velocity of 7,200 ft./sec., so that some 650 ft./sec. had been lost due to drag. Thus one could assume that the two stage version would reach a first stage velocity of $4,470 - 650 \approx 3,820$ ft./sec. By delaying second stage light up there would be reduction in drag in the second stage—one might estimate a drag loss for it of 250 ft./sec. The second stage velocity increment, using the above equation was then $7,650 - 250 = 7,400$ ft./sec., giving a total final velocity of $3,820 + 7,400 = 11,220$ ft./sec. Such a velocity would take the payload to 2×10^6 ft. or about 380 miles.

What did Mr. Hazell consider would be the snags of such a two-stage vehicle? Obviously the short burning time, 3 seconds and the specific impulse of 220 which, with 400 lb. of propellant would give a thrust of 30.000 lb. would impose very severe *g*'s on the instrumentation of the order of 150 compared with 4-10 in Skylark. Also, the second stage would land some 250 miles down range.

Mr. Hazell replied that he had opposed the addition of a second stage to Skylark because of the problems of high *g*, stability, heating and dispersion; however, having raised Skylark to 100 miles, it seemed sensible to use it and accept the difficulties.

He was alarmed a the prospect of 150*g*, but he thought it should be possible to reduce it. From stability considerations it was advisable to try to maintain a fairly constant value of $\frac{1}{2}\rho v^2$. It should be possible to balance the increase of altitude (*i.e.* lower density) against increased velocity by choosing a longer burning time. This would of course lead to a reduction of peak *g* and he thought he could accept values of the order of 50-75g.

The same solution would apply to aerodynamic heating; he would guess at an optimum burning time of 5-10 seconds.

Dispersion was a problem. Scaling up figures for Skylark would give some 60 miles/degree. This could be worse as the two stage vehicle was slower and had a higher payload. A possible solution would be to reduce the burning time of Raven. The dispersion could probably be reduced to 50 miles/degree but could be as high as 80 miles/degree if no remedial action were taken. Wind dispersion would be unaffected at about $\frac{1}{4}°$/ft./sec. The standard error was equivalent to about 4 ft./sec. *i.e.* one degree, so that the vehicle would have a range of 50 miles/degree and a dispersion circle for 1 standard deviation with a radius of 50 miles, *i.e.* the available range would need to be 150 miles with a circle of radius 50 miles around the point of impact. He was not sure this was available.

Spin-resonance and jet-malalignment would have to be examined. He had no reliable figures for jet-malalignment angle for the sort of motor under consideration but he thought it probable that no special precautions were necessary. If difficulties were experienced it was relatively simple to spin the vehicle just prior to the release of the second stage. High altitude separation and ignition and perhaps the dispersion problem would require some thought. This would take time and was why a second stage had been avoided on Skylark in the first instance.

Dr. Boyd commented that three advantages could be gained by adding a second stage to Skylark. These were extra height, extra velocity and the ability to perform some experiments which would ultimately be done on a satellite. The environment would probably be more severe than that of any satellite launched so far. Although most experiments at an altitude in excess of 200 km. could best be carried out by satellites, a vertical sounding rocket was much cheaper and simpler. There was a distinct gap between 100 and 200 km. where such a two stage vehicle would be very useful, *e.g.* in the sphere drop method of measuring atmospheric density; for this to be successful the sphere had to have a high velocity. It had not so far proved convenient to use Black Knight for these experiments as the sphere had to be illuminated by the rising or setting sun and those conducting the sphere-drop tests had no real control over the time of launch of Black Knight.

Very little was yet known about the ionosphere and the outer parts of the F region. Investigation was needed of solar control of the upper F region. No-one knew how rapidly the region varied in its properties, nor the rate at which ionisation fell off. At some unknown height the temperature of the electrons ceased to be in equilibrium with the temperature of the ions. The energy could be perturbed by two effects:

(1) The presence of electric fields in the atmosphere which could give electrons a different energy from the ions.
(2) The production of electrons by ultra violet light from the sun. This gave rise to high energy electrons decaying slowly by impact with neutral particles. As a result there was a disturbance of the mean value of the energy and also of the energy distribution. This problem was almost unexplored and was of importance to the understanding of solar interaction with the upper atmosphere.

The ion energy spectrometer which was to fly in Scout

required rocket or satellite velocity to be large compared with the random velocity of the particles.

In photochemical experiments Dr. Boyd thought little would be gained by using a two-stage vehicle at Woomera. His answer might be different if the experiments could be conducted in the auroral zone, *e.g.* Fort Churchill.

There seemed to be little to be gained by using a two-stage version of Skylark for experiments on geomagnetism or astronomy.

A speaker from Bristol Aerojet gave a description of a vehicle designed and built by his firm. This was called Black Brant (*see* Fig. A) and was in appearance similar to Skylark. The vehicle was being made for the Canadian Government and was essentially a cheap vehicle for testing propellants; to date four had been fired. It was based on a 17-inch motor employing a fairly modern propellant of high specific impulse. An initial acceleration of about 10 *g* enabled the vehicles to be fired from a 15 ft. launcher. The rear structure weighed 145 lb., somewhat heavier than Skylark, but the launching velocity, and hence kinetic effects, were greater than in Skylark. He asked Mr. Hazell firstly whether in the light of his experience at Woomera he considered that the wind dispersion of the Skylark launcher complex (approximately $\frac{1}{4}°$/ft./sec.) was a maximum practical figure for useful work for this type of vehicle; could it be increased and the vehicle still be capable of being fired to high altitude?

Secondly, had Mr. Hazell considered the possibility of using a captive boost? Such a boost would fire for a short period only on the launcher, and would remain on the launcher. In this way launching speed would be increased without imposing extra thrust malalignment during the first wavelength of yaw.

Concerning wind dispersion, Mr. Hazell replied that before the first firing of Skylark it had been the opinion of many at Woomera that it was impossible to allow for it. Fortunately these views had proved incorrect and many Skylark firings had now been made on a basis of the figure of $\frac{1}{4}°$/ft./sec. There was no magic associated with this value but he would not like to see a version of Skylark which was " perfectly all right but has a wind dispersion of $\frac{3}{4}°$/ft./sec." The range, having previously accepted the lower value, would be inclined to ask why it should have to accept a worse figure.

The possibility of achieving higher thrust on the launcher was attractive but the captive boost idea had its drawbacks. The main advantage to be obtained from a captive boost would lie in its use with a large launcher, where it was not really needed. Its use on a small mobile launcher raised problems. How was it to be kept captive?

In response to a question concerning the roll resonance problem on Skylark, Mr. Hazell replied that initially he had been quite satisfied that the problem had been explained by theoretical considerations. However, a factor of some 5 to 10 on the estimate of misalignment and so on, was required before the build up of incidence was large enough to cause the failure that had been experienced. From wind tunnel tests the sharp increase in rate of change of rolling moment with incidence occurred at some 5 to 7 degrees, the rolling moment being equivalent to about 0·1 degrees of incidence on one panel of the fins.

A speaker from R.A.E. Farnborough suggested the real difficulties of a two-stage vehicle lay in the firing of the second stage. The direction of the first stage would need to be rigorously controlled at a time when roll resonance troubles were likely to occur. The problem was aggravated by the fact that the declared width of the range at Woomera was approximately 200 miles. It was likely to prove difficult to contain with ± 100 miles a vehicle having a dispersion of 60 miles/degree.

Mr. Hazell agreed that the availability of range was a problem; however, he thought that firing the second stage accurately was not so much a problem of initial direction, as one of keeping the stage stable. This was a subject on which considerable development was required. He did not think the difficulties were insurmountable; it would not be necessary to incorporate a control system in the 2nd stage.

The same speaker from R.A.E. commented on the difficulties of recovering data. He thought that this was much easier than was generally supposed. To recover a capsule from a satellite required that it should be decelerated by some 3-5,000 ft./sec.; it would then be equivalent to an I.C.B.M. at re-entry. Such re-entries had been successfully accomplished, it being possible to keep instrumentation, including photographic plates, cool.

He suggested that problems were likely to arise in vehicle guidance and in making sure that retro-thrust was applied in the right direction at the right time. This inferred some degree of stabilisation which might prove more dffiicult to achieve than guidance during retardation.

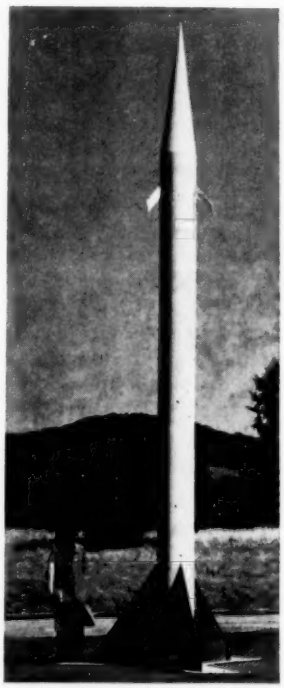

FIGURE A. Black Brant designed and built by Bristol Aerojet in the U.K.

On Black Knight much difficulty had been caused by outgassing. Although the thrust nominally ceased at an altitude of 60-80 miles, it was still possible to measure a residual thrust from the engine at up to 200 miles (caused by boiling off H.T.P.). It was thought that this outgassing continued at heights up to 500 miles and down to the ground again.

On the advantages of recovery, Dr. Boyd commented that he considered that scientifically almost any data could be relayed by devices contained within the vehicle and it was very often not worth trying to recover. Only two classes of experiment existed which really required data to be recovered. These were biological experiments and those involving nuclear emulsions.

Outgassing had been a great nuisance. In trying to avoid it instruments had been mounted on probes, thrown out on yards of cable, or detached from the vehicle altogether, but gases were still given off from the surfaces of metals or from inside pressure gauges; it thus seemed impossible to avoid outgassing even if the motor were discarded altogether. A satellite would, in this context show advantages over the sounding rocket, its metal parts would be outgassed, but even a satellite produced large scale disturbances in the ionosphere at a distance of a kilometre or more. How much more would it cause a disturbance in its immediate vicinity.

A speaker from Hawker Siddeley Advanced Projects Group disagreed with Dr. Boyd's conclusions on the value of recovering data. He reminded them that the best television transmission was inferior to average photographic reproduction. The pictures of the far side of the moon transmitted from the Russian moon shot were no doubt travesties of the photographs contained within the satellite. He thought there were probably better photographs in the satellite of the back of the moon than had so far been secured from Earth of the front. These were wasted because the satellite could not be recovered. Much thought must be given to problems of recovery, not necessarily of whole satellites, but of instrument packages.

Dr. Boyd was in favour of using photographic

emulsions, but he did feel that these could be read by a television technique at the operator's leisure. It was unfortunate that the Russians had failed to get a second look at their moon photographs. This had been their intention.

A speaker from R.A.E. Farnborough endorsed the plea for the use of recoverable photographic emulsions. In Skylark, attitude was measured by two electronic methods with all the possibilities for trouble inherent therein. University College, London, had had carried on some firings a small X-ray camera, and they had had the foresight to make a pinhole in the cover of this with a small piece of emulsion to receive sunlight admitted through the pinhole. On an occasion when Skylark had been fired and the parachute recovery had failed a tangled piece of aluminium with a small piece of emulsion in it had been dug out from a depth of 14 ft. When developed the emulsion was found to carry a family of 20 lines from which it was possible to deduce with surprising accuracy the inclination of the rocket to the sun throughout its flight. The information was not complete but University College had the data required; he and his colleagues were still struggling with the magnetometer and photocell data to try and make it fit the U.C. curves.

Dr. Boyd commented that if the instrumentation was to be recovered as from a sounding rocket then there was much to be said for the emulsion technique (he, himself, had been responsible for the inclusion of the pinhole camera). When it was a matter of recovering a large part of a vehicle from orbit, there was a risk of putting the whole thing out of action, whereas some repeatable device such as television could continue to be used.

In response to questions about the grenade experiments, Dr. Boyd explained that if the grenades were carried above 100 km. and exploded, the quantity of atomic oxygen in the atmosphere caused the products of the explosion to glow, this glow being visible to the naked eye for some 4 to 5 minutes. The cloud of products of the explosion was distorted by the wind and this distortion of the glowing cloud could be used as a measure of wind shear. When using the experiment to determine the speed of sound each grenade gave an average wind speed and an average temperature between the point of explosion and the ground. If two grenades exploded at close but differing heights, different averages would be obtained from each. The process was one of differentiation and the data obtained were purely local since all differences between successive explosions were due to local conditions. It was not necessary to know the conditions prevailing below the lowest explosion for this particular experiment.

METEOROLOGICAL MEASUREMENTS IN THE UPPER ATMOSPHERE

A speaker from Imperial College sought to endorse Dr. Robinson's view that care was required to avoid using rockets and satellites for measurements which could be made just as well by simpler means. As an example a temperature *v.* altitude curve had been established up to 80 km. before ever a rocket had been fired.

Using the term "climate" in a generalised sense meaning some kind of mean value, he emphasised that the meteorologist was not only interested to know what the climate was, but why it was what it was. This led to the study of the weather, for the one could not be understood without the other. It was not sufficient to measure the weather and take arithmetical mean values; the physical working of the atmosphere on the climatological scale depended on the weather, because the weather produced the climate. To advance the study of meteorology, measurements of temperature, density, winds and some radiation properties had to be made frequently and with considerable accuracy. This meant that, for example, a method of measuring temperature by the Doppler line width of some radiating gas to an accuracy of $\pm 15°K$ was of little use unless it were to be used at levels in excess of anything studied so far.

He thought Dr. Robinson over optimistic in saying that satellites would give better weather forecasting. He doubted whether this were true in the middle latitudes of the Western Hemisphere. It might be true for the inhabitants of Mauritius who could probably do with it, though their concern was more with the storm . . than the forecast.

He did think that satellite television coverage would give better data on which to base a forecast, but it seemed unlikely that it would transform meteorology in the sense of explaining the working of the atmosphere. The atmosphere was a very complicated engine with the sun as a source of power, and a sink. It had never been possible in the past to observe how many calories were entering or leaving the atmosphere at any particular time. All that could be done was to take average values over a year or number of years. It was not known how much solar radiation entered the atmosphere, because any patch of cloud caused 80 per cent of the potentially available radiation to be reflected. However, a satellite could observe the strength of the solar heat source and of the sink of the atmosphere as a function of latitude, longitude and time through the spectrum from 5 to more than 50 microns. This gave the basic variables for the physical study of the atmosphere.

Dr. Robinson replied that he too had originally been very sceptical about using television techniques from satellites in weather forecasting. He now thought that although only a marginal improvement was likely in the middle latitudes of the Western Hemisphere, the rest of the world could look forward to considerable improvement, especially those areas where there were few ships (and these not weather ships) or few meteorological centres. Satellite methods might not be economic but the Americans were going to put up meteorological satellites, and soon. This in itself would raise a problem of disseminating all the information which could be received from one satellite on one television channel.

One ground versus satellite observations he emphasised that ground observations of the properties of the upper atmosphere gave mean values, whereas rockets and satellites gave instantaneous pictures of the atmosphere.

RANGE INSTRUMENTATION PROBLEMS

A speaker from the Royal Radar Establishment commented that from a radar point of view instrumentation could be divided into

(1) Sounding rocket *e.g.* Skylark type of experiments.
(2) Satellite type of experiments.

In case (1) the instrumentation was required to operate over a single point near the earth's surface. The vehicle had a short flight time and carried flares or similar devices for optical tracking. The range was shorter and the chances of recovery higher. Because of this there was a preference for concentrating the instrumentation on the ground. He agreed that there were no problems of instrumentation technology outstanding in this class.

In case (2) radar had a restricted application. Echoes had been obtained from the early Sputniks on the 45 ft. radio telescope at R.R.E. These however were more in the nature of curiosities than scientific observations. It was difficult to acquire the target, one had really to point the telescope in roughly the right direction and wait. At Malvern and Jodrell Bank there was a reasonable measure of range, but the angular discrimination was rather poor. High powers, large dishes and therefore narrow beams were required. The Radio Research Board were contracting for some 60 ft. radar trackers.

He wished to reiterate the views of speakers who had said that a sense of balance was required between the use of rockets and satellites on the one hand and ground-based observations on the other. Much could be done by observing meteors and the aurorae and by making ionospheric soundings. Radio astronomy was capable of development. One direction in which this could be realised was the use of large radars for observing non-coherent scattering from electrons in the upper atmosphere.

Mr. Higgs replied that he hoped he hadn't given the impression that he thought instrumentation technology

needed no further development. He thought that instrumentation was adequate for present needs up to say 500 km. He wanted to emphasise the remarks in his paper about Dr. Willmore's photo-electric theodolite. This instrument was relatively simple compared with the Baker-Nunn camera for whose rôle it was successfully competing. One of the greatest problems in tracking satellites was that stations spread widely over the earth's surface had to be tied in closely both in time and co-ordinates. This often proved expensive, and such coverage could not easily be increased.

FUTURE POSSIBILITIES

A speaker from R.A.E. Farnborough commented on the possibilities of a small continuous burning final stage for a 3-stage satellite launcher as distinct from relighting the final stage to accelerate the satellite into orbit. The very least advantage to be gained from a continuous burning third stage would be the saving in weight on the smaller motor and absence of starting system. The restart system also had to carry the whole weight of the previous stage. Considering Mr. Cornford's typical case, the payload put into orbit was reduced from 1,750 lb. to 600 lb. if orbital restarting were used. Furthermore as the third stage would be increasing in velocity for about 30 minutes, it was easier to make corrections to the trajectory over this longer period with the small thrust than it would be during the short burning time of the orbital restarting system.

Of great importance in studies of launching from Woomera was the direction in which the satellite was to orbit. There was in fact little choice. For optimum payload, the satellite should be launched due east, but this would mean the discarded first stage falling on Brisbane. If the launching were due north the second stage would probably fall in China or Russia. It was also desirable to be able to track the first and second burning stages, although some blanks could be accepted in the middle of the guidance period.

The ambient conditions of the satellite were also affected by its orbit, *e.g.* if fired north-east from Woomera the precession of its orbit would cause it to be at first continuously in the sun for long periods, and then half in and half out of the sun. This complicated the heat balance of the satellite. The difficulty could be avoided either by firing slightly west of north (or west of south) and nearly matching the precession of the orbit to that of the earth round the sun or by firing on a nearly equatorial orbit.

Black Knight experience had shown a continuation of thrust after the motor had nominally shut down. This, although small, was of long duration and produced an appreciable velocity increment. Such an increment could cause one stage to bump another after separation. Some means of avoiding this behaviour had to be devised.

A speaker from University College, London, gave details of the joint American-British Scout programme. The satellite weighed 150 lb. and was of the order of 2 ft. in diameter; the orbit in the first instance would be a polar orbit inclined at some 55°. This would give transits across the U.K., although due to its small size the satellite would not generally be visible to the naked eye. There were some limitations on apogee but these were unlikely to prove critical as many experiments could be conducted at altitudes of less than 600 to 700 miles. Full use would be made of American and Commonwealth telemetry stations around the world, although, as Mr. Higgs had pointed out, even the most elaborate system gave only 7 or 8 per cent of total cover. Because of this it was hoped to make use of tape recording with command readout. If a full British space programme developed, experience on Scout, both in the scientific and engineering sense, would be invaluable. If there were no British space programme then Scout would have given a toe hold which could perhaps be enlarged.

The experimental payload of the first Scout had been agreed and discussions were going on concerning telemetry channels, specification of power supplies, layout of structure and so on. The Americans were providing all these items; the British were to provide the sensing instruments. Anyone who had worked in aircraft, missiles or rockets would appreciate the difficulties likely to be encountered when parts of the team were separated by 3,000 miles.

GENERAL DISCUSSION

A speaker from Hawker Siddeley Advanced Projects Group commented that it was necessary to keep in sight the objective of instrumentation, *i.e.* to put a man into space. Instrumentation could only examine phenomena which were known to exist; anything unknown must remain unknown as long as reliance was placed on instrumentation alone.

One had to maintain a sense of balance; the present space programme was scientific, but he wanted to put in a plea for non-scientific satellites for uses quite distinct from the scientific exploration of the upper atmosphere. Examples were reconnaissance and communications satellites. Commercially the latter could compete with a trans-Atlantic cable. The present research programme was costly and although the results were interesting he wondered if the taxpayer had had his money's worth.

Dr. Boyd disagreed that the objective was to put man in space; the objective was to learn about man's environment. When it came to exploring surfaces of other planets then a man would be required, the lines of communication were too long otherwise to control exploratory equipment. Man's five senses, and more, could be covered by instrumentation; he felt that the present limited funds should not be squandered on an adventure, putting man into space.

A speaker from R.A.E. Farnborough commented on the problems of communicating with satellites from earth stations. Accepting that it was now possible to propel a probe or satellite to a considerable distance from the earth conflicting requirements arose in that it was desirable to keep the power requirement in the satellite as low as possible and at the same time maintain long range reception. On the ground there were engineering and probably economic limitations on the aerial arrays which could be provided, although much had been done, *e.g.* the Jodrell Bank aerial. On the satellite, however, many of the aerial arrays in common use were crude in the extreme, largely because initially satellites were not stabilised. It was easier to increase the effective radiating power of a satellite aerial if the satellite could be stabilised, than to increase the effective receiving area of one of the large paraboloid aerials.

Concerning chemi-luminescence, as a result of a system of pyrotechnic and electronic flashes which had been devised to aid the observation of a sounding rocket, it had been possible to put a figure of 90 to 160 km. on the band of altitude at which chemi-luminescence occurred.

Mr. Cleaver, summing up, said that the papers had emphasised the importance of studying the upper atmosphere and beyond. Here, there was an immense natural laboratory with phenomena occurring under conditions which it was difficult or impossible to reproduce on the ground. The Skylark and Raven were a modest British beginning but had given most interesting results. But he felt the work could not end there; there must be a further British contribution. Sending up British instruments in an American vehicle was not entirely satisfactory. The Americans had been most generous, and the offer had to be used to its best advantage, but the U.K. must think of doing something on its own and providing a lead for other interested scientific bodies in the Commonwealth and in Europe. He hoped that the R.A.E. design studies on the Blue Streak-Black Knight combination would come to fruition.

It had been said that the main interest in space research was romantic. There was a romantic element in it, and there was nothing wrong with that, but there was also an element of immediate practical importance in the great strides to be made in the science of meteorology and in telecommunications.

In conclusion, Mr. Cleaver thanked all those who had given papers or taken part in the discussion. He also thanked the R.A.E. for mounting the exhibition of rocket instrumentation on view in the foyer, and the Royal Society for permission to use the material of the exhibition.

The Certification of Civil Transport Rotorcraft with particular Reference to Multi-Engines

by

H. E. LE SUEUR, A.F.R.Ae.S.

1. Introduction

For the past 150 years man's ingenuity has been exercised in producing machines to transport himself faster and faster until today he is talking of aircraft which can travel faster than the rotating speed of the earth at the equator, and such that any point on the earth's surface is within ten hours flight time from any other point. But such is the contrariness of the situation that these speeds involve the use of extensive runways away from the crowded city centres and, what with the crowded roads and trains, the journey from the airport to the city centre can be long, tedious and uncomfortable.

The helicopter enthusiast suggests that this problem can be overcome by using a machine that can take off vertically from the city centre and carry the passenger rapidly in comfort to the airport. He also suggests that for journeys up to 200 miles or more, the helicopter is the quickest form of transport from city centre to city centre.

It is the responsibility of the Airworthiness Authority to ensure that such a journey is safe, or at least as safe as it would be by any other available form of transport. In a modern world it is extremely difficult to establish an acceptable safety level and it is suggested that, as the helicopter has the ability to land vertically without forward speed, it is permissible to relax standards of safety in other fields. I do not agree with this, but would postulate that we should set our standards as high as we can and accept any bonus accruing.

I do not believe that anyone would wish me to spend this lecture describing a programme of certification, but I would like to present some of the problems involved in such certification.

The prime aim of the Airworthiness Authority is to avoid accidents, and the causes of accidents can be divided into several groups. So I would like to examine past helicopter accident analyses and discuss ways and means of ensuring that the accident rate for helicopters is kept to a reasonably low level.

NOTATION

n	number of engines fitted
P_0	power required to maintain level flight at best rate of climb speed
P_H	power required to hover
P_M	power required for maximum level speed
N	number of cycles to failure
$\bar{X}$	mean value of population
σ	standard deviation of population

Appendix I

n	number of engines fitted
r	number of engines failed
${}_nP_r$	probability of r failures out of n fitted
${}_nC_r$	number of combinations of n quantities taken r at a time
ρ	engine failure rate/hour
T	time of flight (hours)

Appendix II

F	safety factor
f_A	allowable stress for N cycles
f_L	fatigue limit stress ($N=10^9$)
N	number of cycles to failure
p	probability of failure
x	number of standard deviations required to achieve p
$\bar{X}$	mean of infinite population
σ	standard deviation

2. Accident and Incident Rates

Information has been collected on helicopter accidents over a number of years. Upon examination it is found that the information is by no means complete, although there is sufficient to determine certain conclusions.

Table I shows the number of helicopter accidents which have occurred in several types of operation, the hours flown, together with accident rate per 1,000 hours of flying. From this table it will be noticed that the accident rate for Scheduled Operation is six times better than for General Operation.

Comparing the figures with aeroplane experience we find a similar accident rate for aeroplanes engaged on private and agricultural work as for helicopters, and as the bulk of helicopter operation has been engaged in similar work, including training, the figures compare favourably and the scheduled figures indicate the improvement that is likely to occur if existing types of helicopter are used for scheduled transport.

The causes of accidents can be divided into four main groups:—

(*i*) Crew Error.
(*ii*) Lack of Mechanical Reliability.
(*iii*) Lack of Performance (Engine Failure).
(*iv*) Any other cause, or unknown.

Table II shows the division of single-engined helicopter accidents divided into these four main groups. It should be noted that the largest cause of accidents in general operation is "Crew Error," whereas in scheduled operation the major cause is the lack of mechanical and engine reliability.

The 6th lecture to be given before the Rotorcraft Section of the Society—on 13th January 1961.

TABLE I

SINGLE-ENGINED HELICOPTER ACCIDENTS

Type of Operation	*1000 Hours Flown*	*Accidents*		
		Total	*Rate*	
General	182	139	0·76	Major
Scheduled	101	12	0·12	
General	60	99	1·67	Minor
Scheduled	101	41	0·41	

Combining Tables I and II we obtain Table III which includes figures for a modern air liner for comparative purposes. From these figures we come to the conclusion that:—

(*a*) The large number of crew error accidents are likely to be reduced to a reasonable level in scheduled operation.
(*b*) Mechanical reliability of helicopters is too low.
(*c*) Engine reliability of helicopters is too low, and multiplicity of engines will reduce the number of accidents due to engine failure.

What have we to learn from this information?

2.1. CREW ERRORS

As in most fields of human endeavour the more the pilot gets to know about his machine and its limitations, the less likelihood there is of accidents. In public transport operation the likelihood of a crew member committing an error is far less than for other types of operation.

2.2. MECHANICAL DEFECTS

In scheduled helicopter operation mechanical defects contributed to one-quarter of the accidents, and of the 791 incidents examined, 22 per cent were again attributed to mechanical deficiencies, and mechanical deficiencies are in a class that the engineer can help to relieve.

In assessing the airworthiness of an aircraft ways and means must be adopted to ensure that these deficiencies are reduced to a minimum. More will be said of this later.

2.3. ENGINE FAILURES

Accidents due to engine failure on helicopters have been extremely high in number for two reasons:—

TABLE III

MAJOR ACCIDENT RATES/10,000 HOURS DIVIDED INTO CAUSE GROUPS

	Type of Operation	*Total*	*Crew*	*Mechanical*	*Engine*	*Other*
Helicopter	General	7·6	3·6	1·3	0·8	1·9
	Scheduled	1·2	0·1	0·3	0·7	0·1
	Modern Aeroplane	0·3	0·1	0·1	0·02	0·1

TABLE II

DIVISION OF ACCIDENTS INTO CAUSE GROUPS

Type of Operation	*Total*	*Crew*	*Mechanical*	*Engine*	*Other*	
General	639	247	122	95	175	Major
Scheduled	12	1	3	7	1	
General	99	15	36	39	9	Minor
Scheduled	41	1	13	27	—	

(*i*) Helicopter operation has been almost exclusively single-engined.
(*ii*) Engine failure rates in helicopters have been high compared with engine failure rates on fixed wing aircraft.

In assessing aeroplane airworthiness it is assumed that the rate of engine failure is approximately one in 2,000 hours of operation. Evidence proves this to be reasonable for reciprocating engines and the trend with turbines is to improve this figure. For helicopters there is no reason why the same figure should not apply providing the engines are suitably tested to ensure their airworthiness. To decrease the number of accidents due to engine failure multiplicity of engines will provide a necessary answer. How many engines are required and the associated powers is the subject of the next Section.

3. Engine Failures and their Effect on Performance

3.1. TAKE-OFF

If we assume that the minimum acceptable rate of engine failure during take-off is $7{\cdot}5 \times 10^{-5}$/engine/take-off, and that reasonable rate of unacceptable loss in performance is 1×10^{-5}/take-off, we establish the following results (*see* Appendix I):—

(*a*) For single-engined helicopters the take-off must always be scheduled so that in the event of an engine failure a safe landing may be made at any stage. This will involve the use of the well-known avoid curve and necessitate operation from open sites.
(*b*) The necessity to schedule for more than one engine failure is not apparent until more than 42 engines are fitted.
(*c*) If the engine failure rate is higher than this figure, then for twin-engine operation the double engine failure during take-off is significant when engine failure rate is higher than 3×10^{-3}/take-off or one in 300 take-offs.

3.2. CRUISE

For cruise we can assume an engine failure rate of 5×10^{-4}/hour/engine and the same reasonable rate of unacceptable loss in power of 1×10^{-5} per flight. With these assumptions we find, from Appendix I,

(*a*) Single-engined helicopters must be so routed that

at any time during the flight a safe landing can be made in auto-rotation.

(*b*) For multi-engined helicopters it is necessary to schedule the flight for single engine failure during cruise.

(*c*) The time at which double engine failure becomes significant is a function of the number of engines fitted and is plotted in Fig. 1.

3.3. LANDING

When a helicopter uses restricted sites its dependability on its engines will be greater than is normal with an aeroplane (apart from balked landing). Therefore landings with helicopters will need to be scheduled with one engine failed, and the following rules should be noted:—

(*i*) Single-engined helicopters must land in open sites.

(*ii*) Multi-engined helicopters must be scheduled to land with one engine failed.

(*iii*) If the flight time is such that double engine failure in cruise is significant (*see* Fig. 1)

(*a*) then they must be scheduled to land with two engines out; or alternatively

(*b*) if an engine fails *en route* they must be diverted to an open site.

3.4. ENHANCED POWERS REQUIRED FOR PERFORMANCE IN CRUISE

To cover the event of engine failure it is necessary to enhance the powers of the unfailed engine. If we assume no loss in power, then the enhancement factors are as in Fig. 2. However, if it is assumed that the powers required in the various stages of flight are related such that

$$P_H = 1{\cdot}5P_C$$

$$P_M = P_H$$

where, P_H is power required to hover;

P_C is power required to maintain level flight at best rate of climb speed;

P_M is power required for maximum level speed;

we find that the power factors required for cruise over and above that required for maximum speed are as shown in Fig. 3. These power requirements would normally fix the design for the helicopter.

3.5. ENHANCED POWER FACTOR REQUIRED TO HOVER

If we postulate that the power required to hover (P_H) is equal to power required for maximum speed (P_M) then with the engine failed case the enhanced power factor required to hover with engines failed follows the curves of Fig. 2. For take-off the curve for one engine failed applies but we must still consider the curve for two engines failed in landing if the flight time renders it significant.

4. Contingency Powers

4.1. THE TWIN

Referring to Fig. 3 the power required to maintain height at best rate-of-climb speed, with one engine out, is 1·33 times the power required for maximum speed,

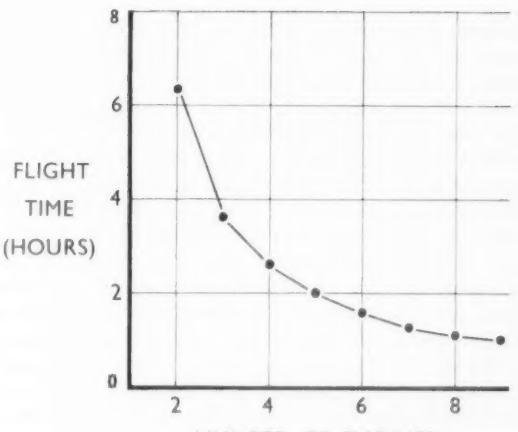

FIGURE 1. Significant flight time for double engine failure.

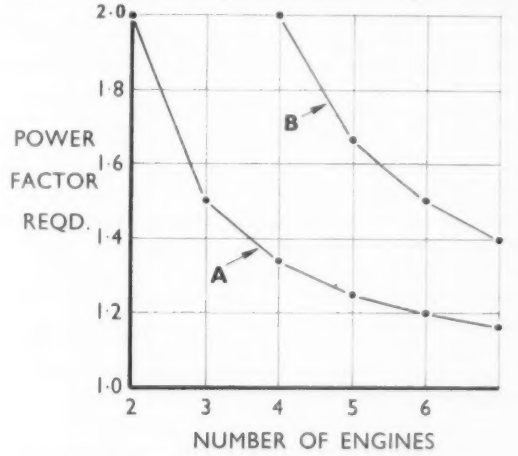

FIGURE 2. Power factor required for no power loss.
A One engine failed. B Two engines failed.

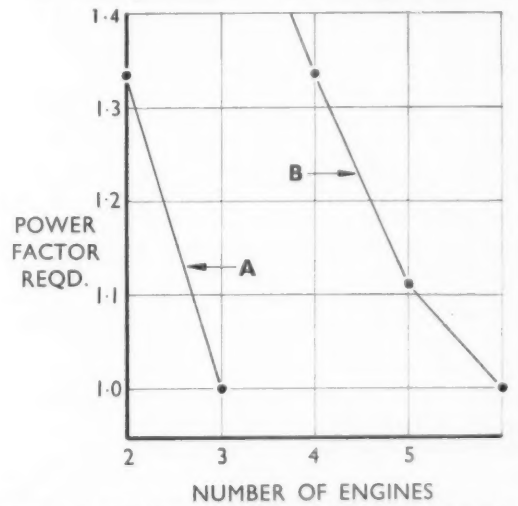

FIGURE 3. Power factor required for maintaining level flight at best rate of climb speed (above power required for maximum speed all engines operating).
A One engine failed. B Two engines failed.

and from Fig. 2 the power required to hover with one engine out is 2·0 the power required for maximum speed. Therefore, the power required to hover with one engine out is 1·5 times the power available if the one-engine-out in cruise case fixes the design.

On an average the power available from an aeroplane engine at Take-off Rating is between 1·15 and 1·20 times the Maximum Continuous Rating. However, it must be remembered that for aeroplanes take-off power is used in every flight to reduce the length of the runway.

For helicopters, the take-off can be achieved without calling for more than Maximum Continuous Power, provided there is no engine failure. However, if there is an engine failure, as previously stated, the power required on a twin is more than 1·20 times the Maximum Continuous Power. Can the engine manufacturer be asked to supply a higher power than take-off power which will only be used in the event of engine failure? Increase in power for turbines means increase in flame temperature and engine speed, which means higher blade stresses and greater creep, and hence lower turbine blade lives.

However, it is estimated that an increase in flame temperature of 100°C above take-off flame temperature would increase the power by 10 per cent, *i.e.* 30 per cent above Maximum Continuous Power. Provided this power is required for not more than 2½ minutes at any one time, and for not more than 10 times between overhauls, it should have little or no effect on the turbine blade life and overhaul life as against using take-off power in an aeroplane at every take-off. This increase is not all that is required for a twin, but if such an increase were available, it would help the designer.

4.2. MULTI-ENGINED HELICOPTERS AS DISTINCT FROM TWINS

The argument on the necessity for contingency powers for multi-engined helicopters can be extended to any number of engines. Fig. 4 shows the power factors

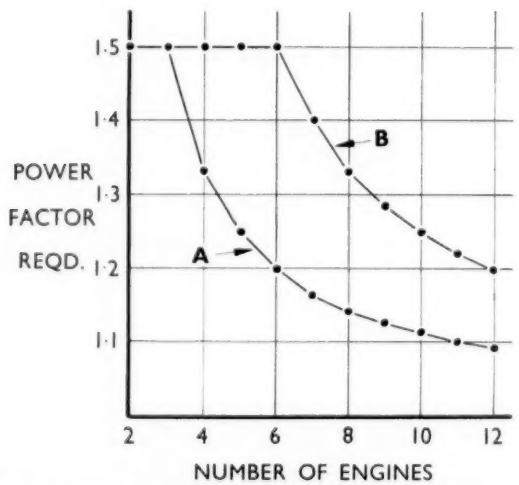

FIGURE 4. Power factor required to hover above power required for maximum speed.

A One engine failed. B Two engines failed.

required to hover. The cut-off at 1·5 assumes that the rotorcraft is designed to maintain height with the appropriate number of engines failed at the best rate-of-climb speed and, as stated before, the two-engine-out case applies to landing after a long flight. The above argument is not meant to apply to any particular rotorcraft or engines; the figures are not exact; they do not include any Statutory margins; they are merely put forward as an example for discussion. If the need for contingency powers is accepted, then the Type Test on the engine would require altering to accommodate them.

5. Performance Requirements

In assessing the airworthiness of civil transport multi-engined rotorcraft the parameters affecting performance are:—

all-up weight
altitude
ambient temperature

and under certain conditions:—

aerodrome gradient.

The wind allowance depends on direction of take-off and landing:

a maximum headwind of 50 per cent
or, a minimum tailwind of 150 per cent
or, still air

must be assumed.

The take-off space, rejected take-off area and landing space required shall be less than that available. The alternatives in take-off necessitate the use of a Decision Point, and up to the Decision Point the pilot shall be able to reject take-off and land safely back on. From and beyond the Decision Point the performance of the rotorcraft with one engine inoperative shall be such that the rotorcraft can clear the boundary of the take-off area by at least 50 ft., and subsequently up to a height of 1,000 ft. clear vertically any obstacle by at least 35 ft. plus one per cent of the distance of the obstacle from the take-off boundary.

An obstacle is any prominence within a horizontal distance from the line of flight of 300 ft. + ⅛th of the distance from the take-off boundary with a maximum of 1,000 ft. on a clear day with a change of heading less than 15° or 3,000 ft. for all other conditions.

5.1. PROPOSED VERTICAL OR NEAR VERTICAL TAKE-OFF

Referring to the upper part of Fig. 5 for the proposed vertical (or backward) take-off the take-off path shall be such that during Stage 0–1 in the event of engine failure the rotorcraft can safely return to a state of rest within the confines of the take-off area.

This will necessitate:—

(*a*) adequate pilot's view of the take-off space,
(*b*) adequate clearance from all obstacles outside the view of the pilot,
(*c*) a maximum vertical rate of descent after engine failure such that when the rotorcraft touches the ground after flare out it does not exceed the normal vertical velocity of descent for landing strength (8 ft./sec.). This vertical rate of descent should be assessed and demonstrated using the

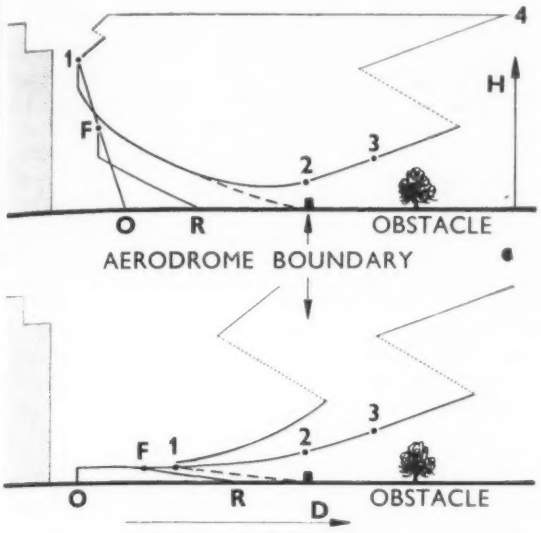

FIGURE 5. Take-off paths.

Maximum Contingency Power on the unfailed engines (Path *F-R*).

Point 1 is the Decision Point and is determined in terms of height (*H*). In the event of engine failure at Point 1 the pilot shall be able

either (*a*) to make a safe landing within the confines of the Take-off Area similar to path *F-R*;

or (*b*) to continue his flight such that Point 2 can be reached without touching the ground.

The requirements at Point 2 are:—

(*a*) at least 50 ft. above the end of take-off space (H_2),

(*b*) The rotorcraft shall be flying at a speed greater than the Take-off Safety Speed,

(*c*) the performance of the rotorcraft shall be such that with Maximum Contingency Power on the unfailed engines Point 3 can be reached.

At Point 3 the rotorcraft shall be:—

(*a*) within 2½ minutes of Point 1,

(*b*) at the appropriate height to avoid obstacles.
$H_3 > H_{ob} + 35$ ft. $+ 0{\cdot}01\ (D_3 - D_2)$
where H_{ob} is height of obstacle at $D_3 - D_2$ ft. from boundary,

(*c*) able, with intermediate contingency power (or Maximum Continuous Power) on the remaining engines to reach a height of 1,000 ft. (Point 4) avoiding all obstacles with a vertical clearance of 35 ft. + one per cent of the distance from the take-off boundary.

5.2. NORMAL TAKE-OFF

The normal technique for take-off which can be used by both single and multi-engined rotorcraft is shown in the lower part of Fig. 5 and involves hovering in the ground cushion and accelerating horizontally forward to a speed V_1 (the Decision Point) such that during the phase 0–1 the rotorcraft can be safely landed back-on within the rejected take-off area available. At Point 1 the pilot either has the option of landing back-on or proceeding to the Point 2, *i.e.* at Point 1 the rotorcraft must have such performance that Point 2 can be reached with one engine inoperative. From Point 2 the requirements are the same as for the proposed vertical take-off.

A normal take-off will be much safer than the proposed vertical take-off, the pilot should have a good view during the whole manoeuvre, there should be little or no downward vertical movement of the rotorcraft following engine failure. However, although the rotorcraft will be able to start its run much closer to the near end of the take-off space available the overall length of take-off space and rejected take-off area required is likely to be much larger than that required for the proposed vertical take-off. Furthermore the speed of touchdown in the rejected take-off is likely to be higher.

For multi-engined helicopters the performance of which falls below the above requirements there is a maximum weight limitation of 20,000 lb. and the height clearance at Point 2 shall be 100 ft. There will obviously be no Decision Point for such rotorcraft, and the rotorcraft should be flown such that a safe landing can always be made in the event of engine failure.

5.3. EN ROUTE

At the appropriate aircraft weight and in the appropriate meteorological conditions with one or more engines inoperative depending on the flight time (*see* Fig. 1) the available gradient or climb shall be not less than 0·5 per cent or alternatively the rotorcraft shall be so flown that in the event of engine failure it can continue its flight to an aerodrome where it can make a prescribed landing.

The height clearances shall be 2,000 ft. for obstacles within 5 nautical miles, except that with visibility greater than 2 nautical miles then those obstacles farther than 2 nautical miles can be ignored.

5.4. LANDING

For helicopters using a restricted site then providing the Point 1, for take-off, can be reached with all engines operating the helicopter can descend down the same path 1–0 or, in the event of subsequent engine failure, *F-R*, or similar paths assessed for landing weight.

Should an engine failure occur before Point 1 in the landing is reached then the probability of a second engine failing will need to be considered, particularly if the flight time exceeds the significant periods shown in Fig. 1. In the event of an engine failing during cruise or let down, the pilot may have to consider whether it would not be safer to fly to an alternative open site. Information will be necessary to help him decide.

5.5. GENERAL

Present knowledge on the operation of multi-engined helicopters is severely limited and the majority of the foregoing argument is conjecture. It may be that with more information on actual operations the present view will have to be altered.

The proposed airworthiness requirements for multi-engined rotorcraft are written in terms of the given minima. In the requirements, however, it is necessary to consider other items besides the legal minima, *e.g.*

variations in engine powers and so extra increments of performance are necessary. These extra increments are at present under discussion and are therefore not established minima.

The proposed figures are:—

(*a*) net rate of climb at Point 2—100 ft./min.,

(*b*) take-off net flight path shall not fall below a profile extending from the necessary height at Point 2 with a gradient of 3 per cent to a height of 1,000 ft.,

(*c*) at 1,000 ft.,

(*i*) the gross rate of climb shall be not less than 150 ft./min.

or (*ii*) the gross gradient of climb shall be not less than 1·5 per cent,

(*d*) for rotorcraft not having a scheduled engine-out performance the performance available shall be equivalent to a gradient of climb of 8 per cent with all engines operating at Maximum Continuous Power.

6. Mechanical Reliability

As indicated the rate of accidents in helicopters due to mechanical faults has been high. Particular attention is required for that part of the helicopter which corresponds to the primary structure of an aeroplane, *i.e.* that part which provides the lift and enables the pilot to maintain control of the aircraft. In the helicopter this part is the rotor system and includes:—

(*a*) the rotors, including hubs,

(*b*) the part of the transmission necessary to maintain the rotors in phase or running at the requisite speed for control,

(*c*) the part of the control system that rotates with the rotors,

(*d*) the static part of the control system including the power system, if powered controls are used,

(*e*) the supporting structure.

It is difficult to find a parallel in other branches of engineering where so much depends on the reliability of moving parts and it is therefore necessary to consider the rotor system, and its supporting structure in detail to determine a method of assessing the reliability.

In aircraft design when it is known that the failure of a part is likely to be catastrophic the practice is to duplicate or multiplicate to reduce the effects of a single failure to a reasonable level. However, with a helicopter there are some parts which at present do not lend themselves to duplication or multiplication, such as the rotor system. How can we ensure that such a system has the reliability required of a transport vehicle?

TABLE IV

ALUMINIUM ALLOY PARTS $N=10^9$

Order of Vibration	*Speed r.p.m.*	*Flying Hours for Unlimited Fatigue Life*
Rotor	250	67,000
Reciprocating Engine	2,000	8,000
Turbine	10,000	1,700

7. Structural Integrity

The maximum normal manoeuvring accelerations so far measured in flight have occurred during training and have been of the order +1·4 (flare out) and −1·2 (start of descent). These represent a maximum n of +2·4 and −0·25. These figures justify the present requirements of $n=3·0$ (reducing to 2·0 if agreed) and −0·5.

Corresponding landing normal accelerations have been +1·4 and −0·9 ($n=2·4$ and +0·1). Gust effects for flying in fair weather conditions gave rise to normal accelerations of +1·0 and −1·1 ($n=2·0$ and −0·1). For all-weather flying increased gust effects are likely to occur and it would appear that there is some justification for extending the gust envelope up to that required for aeroplanes: to wit, the equivalent of a 66 ft./sec. gust.

7.1. FATIGUE

The fatigue aspects of structural integrity have caused some concern in the aeronautical world of late years. However, for helicopters the problem has been recognised from the start and measures have been taken to ensure that accidents due to fatigue in helicopters should be remote. The complexity of the helicopter has resulted in some failures and some thought must be given to the problem before the multi-engined helicopter is allowed to move freely in its proposed orbit, especially as the helicopter as at present envisaged does not allow for fail-safe design.

It is assumed that the fatigue limit of light alloys corresponds to one thousand million reversals, *i.e.* if the structure can withstand 10^9 reversals of the most severe applied loads, then the structure has an unlimited fatigue life.

With a helicopter using a rotor speed of 250 r.p.m. for stresses of first rotor order, this number will be reached in 67,000 flying hours. For other orders of vibration corresponding figures are shown in Tables IV and V. From these figures we can draw certain conclusions:—

(*a*) engine parts (especially turbines)—
Design and establish unlimited life.

(*b*) rotor system parts in Aluminium alloy—
Design to fail-safe and establish reasonable safe life in each part,
or, establish safe fatigue life,

(*c*) for low grade steel rotor system parts—
Design and establish unlimited life.

(*d*) for high grade steel rotor system parts where the corresponding number of cycles to failure can be as high as 10^8, alternative (b) may be necessary.

TABLE V

STEEL PARTS $N>5+10^6$

Order of Vibration	*Speed r.p.m.*	*Flying Hours for Unlimited Fatigue Life*
Rotor	250	>350
Reciprocating Engine	2,000	> 42
Turbine	10,000	> 9

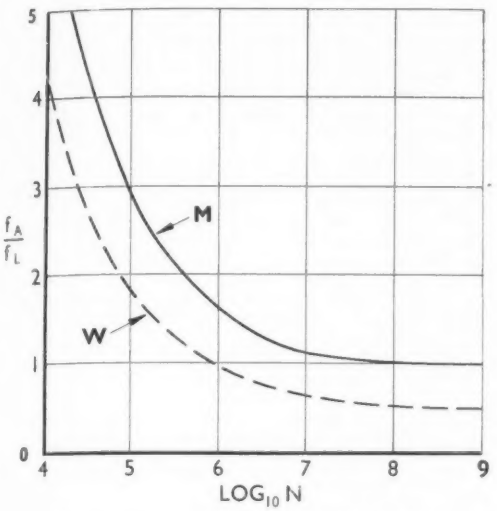

FIGURE 6. Fatigue *S-N* curves for light alloy.

M = Mean curve from test
W = Working curve
f_A = Allowable stress for *N* cycles
f_L = Fatigue limit stress

7.2. SAFE FATIGUE LIVES

Arguments have arisen as to whether a long or short fatigue life is the more airworthy. This is apart from the economic problem of continued replacement of parts with low life.

With long fatigue lives the question of wear and corrosion and their effects on fatigue must ultimately arise. It is postulated that regular overhaul can obviate wear provided the effect of tolerable wear is known and regular inspection should control corrosion, provided the design allows for this.

In the establishment of safe fatigue lives it is necessary to ensure that the factors of safety that are applied achieve the desired result.

It is generally agreed that the factor for scatter from rotorcraft to rotorcraft and effects of wear should be of the order of 1·2. There are no statistics to confirm or deny this and so it must stand until such time as it is proved to be too high or too low.

The material and assembly factor can be investigated. If we examine a typical *S-N* curve for light alloy (curve *M* in Fig. 6) such a curve can be obtained by testing the component at different levels of vibratory stress. It is required to know what is a reasonable working curve (*W*) to obtain a reasonable level of safety. The shape of the working curve will depend on,

(*a*) the vibratory stress level to be expected in service;
(*b*) the frequency of vibration of the fatigue damaging stress level;
(*c*) the confidence that can be placed on the test results.

Examination of the results of fatigue testing lugs summarised in Ref. 2 show the following results.

For Light Alloy Lugs at any particular value of cycles to failure (*N*) assuming a population of log-normal distribution of stress, then the results will have a distribution about the mean $(\overline{X})$ with a standard deviation (σ) equal to 0·012 log *N*, with a mean value 0·965 of the experimental mean assuming a confidence of 90 per cent.

If we consider three conditions:—

(*i*) a constant probability of failure of 10^{-7}/hour,
(*ii*) a probability of failure increasing with increase of life such that at failure the probability of failure is the life in hours divided by 10^7 associated with a constant rate of application of 250 cycles per minute, *i.e.* a probability of $0{\cdot}67N/10^{11}$, where *N* is the number of cycles to failure,
(*iii*) a safe life of 10,000 hours with infrequent vibrations and a probability of failure at safe life of 10^{-3}, *i.e.* an overall probability of 10^{-7}/hour.

Then the factors to be applied to the *S-N* curve are as in Fig. 7 and the working curves are as in Fig. 8. For clarity the values are shown in Tables VI and VII.

These are only three examples and are applicable to light alloy. Similar curves can be obtained using the information from test results for steel, and for other conditions for light alloy by a similar process.

These factors may cause some consternation to those concerned with the establishment of safe lives, and to demonstrate the effect two examples are necessary.

Example I

Using the accepted method of factoring the *S-N* curve by 1·2, the apparent safe life of a part is established as 10,000 hours, with damaging cycles of 10 per hour.

What is the reliable safe life?

Using the method of Appendix II, the reliable safe life is 4,200 hours.

Example II

A transport rotorcraft with a rotor r.p.m. of 250 has a part with an apparent safe life of 10,000 hours, based on the stress measured at V_{NO} operating for 80 per cent of its life at this speed.

What is the reliable safe life?

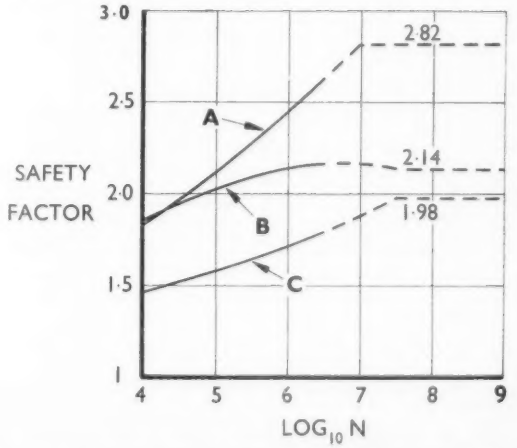

FIGURE 7. Safety factors required to achieve probability of failure less than one in 10^7 hours.

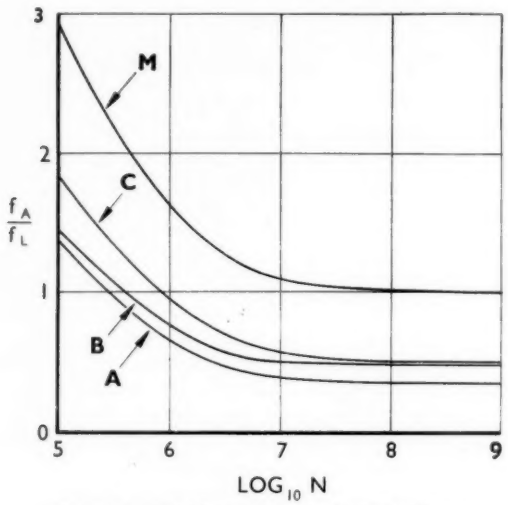

FIGURE 8. Working *S-N* curves for light alloy.

Using the method of Appendix II, the reliable safe life is 40 hours. This is ridiculously low and therefore a re-assessment of the situation is necessary. We cannot question the variability in test results.

Assuming we accept the original estimate of 10,000 hours, the probability of failure in the last hour of the established life is 1/4, which is unacceptable. (*See* Appendix II.)

Reducing the safe life to 2,500 hours does not alter the probability of failure, and therefore the only conclusion to be deduced is that for all normal level flying with vibration frequency of rotor order it is necessary to design for infinite life with a safety factor of 3·0 for light alloys (Curve *A*).

Steel parts require a separate investigation, but the evidence of Ref. 2 indicates that scatter is the same, and so similar factors are required for steel parts with unlimited life.

The factors quoted are safety factors for lugs not concentration factors. The use of force fits, pre-stressing, and so on, may well improve the fatigue limit stress but there is no evidence to suggest that such devices will improve the scatter on results.

8. Transmission Reliability

When considering the transmission of a helicopter a new problem arises. The endurance test on engines has resulted in a reliability approximating to a failure rate of one in 2,000 hours or 6,000 engine take-offs, or better for fixed-wing aeroplanes. From our analysis of accidents we establish that engine failures have caused an accident rate of one in 2,200 hours for single-engined helicopters.

Ignoring the effects of heat it can be assumed that, if the transmission is subjected to a similar endurance test as is prescribed for the engine, the same order of reliability will result. This is obviously not enough for multi-engined helicopters if failure of the parts of the transmission would cause or lead to catastrophe.

Examining the transmission of the conventional

TABLE VI
LIGHT ALLOY SAFETY FACTORS

N	*Safety Factor*		
	$p=\frac{1}{10^7}$	$p=\frac{0{\cdot}67N}{10^{11}}$	$p=\frac{1}{10^9}$
10^4	1·84	1·85	1·46
10^5	2·12	2·02	1·58
10^6	2·45	2·14	1·71
10^7	2·82	2·16	1·88
10^8	3·25	2·10	2·05
10^9	3·76	1·91	2·23

TABLE VII
LIGHT ALLOY ALLOWABLE STRESSES

N	*Mean Vibratory Stress/ Fatigue Limit*	*Allowable Stresses/Fatigue Limit*		
		$p=\frac{1}{10^7}$	$p=\frac{0{\cdot}67N}{10^{11}}$	$p=\frac{1}{10^9}$
10^4	6·35	3·44	3·43	4·36
10^5	2·92	1·38	1·45	1·14
10^6	1·62	0·66	0·76	0·95
10^7	1·09	0·39	0·50	0·58
10^8	1·01	0·35	0·48	0·50
10^9	1·00	0·35	0·48	0·50

helicopter results in finding a large number of parts, the failure of which could cause catastrophe, and the question arises: What standard of reliability is required for such parts, and what are the available means of establishing such standard? A large number of the mechanical accidents to single-engined helicopters have been due to failure of the transmission, and therefore the present standard of reliability is insufficient.

The probable causes of transmission failure are:—

(*a*) undue wear,
(*b*) over-stressing due to
  (*i*) over-torquing (uneven engine running),
  (*ii*) clutch snatch,
  (*iii*) over-speeding (auto-rotative and engine surge),
  (*iv*) excessive vibration (resonance),
  (*v*) undue transfer of power to tail rotor during the execution of certain manoeuvres,
(*c*) bad or insufficient lubrication,
(*d*) mishandling of the controls.

During the prototype testing it is therefore necessary to ensure that all these points are catered for in order to acquaint the responsible personnel of the probable result of exceeding the limitations of the machine.

The test programme should include the following:—

(*a*) Endurance Testing.
(*b*) Vibration Analysis (including resonance).
(*c*) Fatigue Testing.
(*d*) Abuse Tests.

The Endurance Testing should be so weighted a sufficient number of times and for sufficient durations to cover any condition that is likely to occur during the overhaul life of the transmission. As a result of the endurance testing the replacement times of bearings and wear tolerances on teeth and shafts may be determined

The Vibration Analysis should be conducted over the full range of air speed, engine speed, rotor speed, aircraft weight and centre of gravity, with appropriate margins to ensure that possible exceeding of the permissible ranges does not adversely affect the airworthiness.

The purpose of Fatigue Testing is to establish safe fatigue lives for those parts, the failure of which would be catastrophic and to ensure that other parts, the combined failure of which would be catastrophic, are so lifed as to make the possibility of catastrophe extremely remote. In this respect the arguments about fatigue under "Structural Reliability" equally apply. It is usual for the majority of parts in the transmission to be made of steel, and therefore a knowledge of the effects of scatter is required. It is generally accepted that low grade steels reach a fatigue limit after 5×10^6 cycles, whereas for high grade steels a figure of 10^8 cycles could well apply.

There is no evidence to suggest that the effects of scatter are any less for steels than for light alloy and therefore the same factors are required over the appropriate cycle frequency ranges. In assessing the fatigue reliability of helicopter transmission we could well take a lesson from propeller manufacturers who design for unlimited life with a safety factor of 2·0 on the fatigue limit with subsequent high life fatigue testing, *i.e.* retiring specimens at specified intervals (say, 2,000 hours) and fatigue testing the retired items. This may be costly, but no adequate alternative has yet been proposed.

The curve in Fig. 9 shows the safety factor required against probability of failure for a specimen having a mean value of $\overline{X} = 0{\cdot}989$ (antilog $-0{\cdot}01$) and a standard deviation of $0{\cdot}01 \log N$ where N is $3{\cdot}16 \times 10^6$, *i.e.* $\sigma = 0{\cdot}065$.

The Abuse Tests should include such items as:—

(*a*) excessively rapid clutch engagements,
(*b*) operating the rotorbrake with the transmission running at high speed,
(*c*) starting up the rotor with the brake engaged,
(*d*) running the transmission with inadequate lubrication,
(*e*) running the transmission with inadequate cooling of the lubricant,
(*f*) over-speed and under-speed tests.

In considering the necessity for abuse tests it must be remembered that we are dealing with a transport vehicle which is likely to receive some rough treatment at some time during its life and particularly in training. As is well known, the effect of abusing a piece of machinery on the fatigue life of its parts is extremely difficult to assess.

9. The All Weather Helicopter

Until now the helicopter has been essentially a fair weather machine, its activities being confined to VFR or VMC. For the multi-engined helicopter to be a successful transport vehicle it must maintain schedules and fly in bad weather. This will necessitate equipment to enable such flying to be undertaken. Such equipment

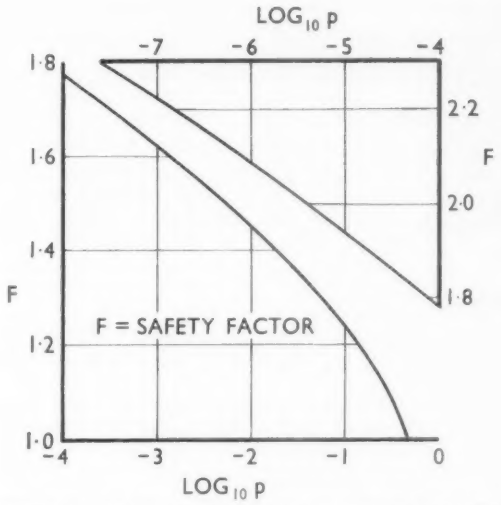

FIGURE 9. Safety factor required for a given probability ($X = 0{\cdot}977$, $\sigma = 0{\cdot}065$) log-norm distribution for stress.

will probably include:—Auto-stabilisers, Flight Directors, Blind Flying Instruments, De-icing Equipment, Ground Aids.

The ability of the helicopter to fly at low speeds will necessitate that the equipment functions adequately and safely at such speeds as the flight planning demands. It is pleasing to know that experiments are in hand to ensure that the right type of equipment is used, and that the reliability of the equipment is of the order required.

10. Other Aspects

In this paper nothing has been said of those aspects of certification of helicopters which are equally applicable to aeroplanes. The standards required are well known and it is sufficient merely to list some of them.

(*a*) Crew Compartment Design. (*Note*: Pilot View requirements may be different.)
(*b*) Passenger Compartment Design, including seats and aisles. (Crash factors for seats are different.)
(*c*) Escapability and Accessibility.
(*d*) Cargo Compartments.
(*e*) Fire Precautions.
(*f*) Electrical Systems and Bonding against Lightning Strikes and Static.
(*g*) Functioning of Essential Services in the event of Power Failure.
(*h*) Power Plants, including Fuel Systems, Oil Systems, Intakes and so on.
(*i*) Cooling Systems. (*Note*: Cooling required at zero speed.)
(*j*) Essential Instruments. (*Note*: Air Speed Indicator required at low speed for blind flying.)
(*k*) Mandatory information and Publications.

This list is not meant to be exhaustive.

Concluding Remarks

I am sorry if I have appeared to make the certification of multi-engine rotorcraft extremely difficult. The

suggestion put forward on fatigue may cause some concern, but I must plead that if the required reliability is to be achieved, such factors are necessary.

Acknowledgment

The author is indebted to the Air Registration Board for permission to present the paper: the opinions expressed however are personal. Colleagues at the Board and in Industry are thanked for the help they have given.

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APPENDIX I

NUMBER OF ENGINE FAILURES WHICH NEED CONSIDERING FOR MULTI-ENGINED ROTORCRAFT

Take-Off

If we assume that the acceptable engine failure rate during take-off is $7{\cdot}5\times10^{-5}$/engine, and that a reasonable rate of unacceptable loss of power at 1×10^{-5}/take-off, we have for one take-off

$${}_np_r={}_nC_r\,\rho^r$$

where, ${}_np_r$=the probability of the failure of r engines out of total n

$${}_nC_r=\frac{n(n-1).....(n-r+1)}{1.2.....r}=\frac{n!}{(n-r)!\,r!}$$

ρ=rate of engine failure/take-off.

ONE ENGINE

$${}_1p_1=1{\cdot}0\,\rho^1$$
$$=7{\cdot}5\times10^{-5}\quad\text{Unacceptable.}$$

TWO ENGINES

Single engine failure

$${}_2p_1={}_2C_1\,\rho^1$$
$$=2\times7{\cdot}5\times10^{-5}$$
$$=15\times10^{-5}\quad\text{Unacceptable.}$$

It is therefore necessary to legislate for single engine failure for all powered aircraft.

Double engine failure

$${}_2p_2={}_2C_2\,\rho^2$$
$$=1\times7{\cdot}5^2\times10^{-10}$$
$$=56{\cdot}25\times10^{-10}\quad\text{Acceptable.}$$

For twin engines it is not necessary to legislate for double engine failure during take-off.

Note as ${}_2p_2=\rho^2$
Allowable $\rho=\sqrt{10^{-5}}=3{\cdot}16\times10^{-3}$

i.e. if the possibility of engine failure during take-off is higher than one in 300 take-offs the twin-engined rotorcraft must be flown so that a safe landing can be made at any time.

THREE ENGINES

Double engine failure

$${}_3p_2={}_3C_2\,\rho^2$$
$$=3\times7{\cdot}5^2\times10^{-10}$$
$$=169\times10^{-10}$$
$$=1{\cdot}69\times10^{-8}\quad\text{Acceptable.}$$

Minimum number of take-offs/failure

$$=\sqrt{\frac{10^5}{3}}=170.$$

TO DETERMINE THE NUMBER OF ENGINES WHEN TWO FAILURES DURING TAKE-OFF BECOMES SIGNIFICANT, CONSIDER

$${}_np_2={}_nC_2\,\rho^2$$
$$10^{-5}<{}_nC_2\times56{\cdot}25\times10^{-10}$$
$${}_nC_2>\frac{10^{-5}}{56{\cdot}25\times10^{-10}}=\frac{10^5}{56{\cdot}25}=\frac{8\times10^5}{450}$$
$$=\frac{8}{9}\times10^3=889.$$

As ${}_nC_2=\dfrac{n(n-1)}{2}$

$$n>\sqrt{(2\times889)}=\sqrt{1{,}778}=42{\cdot}2.$$

Check

$$n=42\ {}_{42}C_2=21\times41=851$$
$$n=43\ {}_{43}C_2=43\times21=903.$$

Two failures become significant during take-off with 43 engines.

Cruise

For cruise let us assume an engine failure rate of

$$5\times10^{-4}\text{/hour/engine.}$$

TWIN ENGINE

Double failure in one hour flight

$${}_2p_2={}_2C_2\,\rho^2$$
$$=1\times25\times10^{-8}\quad\text{Acceptable.}$$

Note: Engine failure rate for double failure to be significant for one hour flight

=one failure in 300 hours flying.

Again, as $10^{-5}=\rho^2\,T^2$

$$\rho T\not<3\times10^{-3}$$
$$\rho\not<3\times10^{-3}/T$$

If we wish to fly longer the reciprocal of engine failure rate must be increased in proportion to flight time, *i.e.* for 2 hours we require a reliability greater than one failure in 600 hours.

NUMBER OF ENGINES WHEN TWO FAILURES BECOME SIGNIFICANT IN CRUISE FOR ONE HOUR'S FLIGHT

$${}_np_2={}_nC_2\rho^2$$
$$10^{-5}<\frac{n(n-1)}{2}\times25\times10^{-8}$$
$$n(n-1)>\frac{10^{-5}\times2}{25\times10^{-8}}=\frac{2{,}000}{25}=80$$
$$n>\sqrt{80}$$
$$n>8{\cdot}8$$

Check

$$n = 9 \quad n(n-1)=72$$
$$n = 10 \quad n(n-1)=90$$

i.e. 10 engines are significant.

FLIGHT TIME WHEN THE FAILURE OF TWO ENGINES BECOMES SIGNIFICANT.

$${}_nP_2 = {}_nC_2\,\rho^2\,T^2$$

$$T^2 = \frac{{}_nP_2}{{}_nC_2\,\rho^2} = \frac{10^{-5}}{{}_nC_2 \times 25 \times 10^{-8}} = \frac{40}{{}_nC_2}$$

TABLE VIII

FLIGHT TIME WHEN TWO ENGINE FAILURE IS SIGNIFICANT

n	${}_nC_2$	$T^2 = \frac{40}{{}_nC_2}$	T hours
2	1	40	6·3
3	3	13·3	3·6
4	6	6·7	2·6
5	10	4·0	2·0
6	15	2·7	1·6
9	36	1·1	1·05
10	45	0·9	0·95

TABLE IX

ENHANCED POWER REQUIRED TO MAINTAIN PERFORMANCE

Number of Engines	*Power Factor Required*	
	One failed	*Two failed*
2	2·0	—
3	1·5	3·0
4	1·333	2·0
5	1·25	1·66
6	1·2	1·5
10	1·111	1·25

NUMBER OF ENGINES AT WHICH 3 ENGINE FAILURE IS SIGNIFICANT FOR ONE HOUR FLIGHT.

$$T^3 = \frac{10^{-5}}{125 \times 10^{-12}} \times \frac{1}{{}_nC_3} = \frac{8 \times 10^4}{{}_nC_3}$$

i.e. $n^3 = 6 \times 8 \times 10^4 = 5 \times 10^5 = 500 \times 10^3$
$= 80^3$ approx.

or, ${}_nP_3 = {}_nC_3\,\rho^3\,(T=1)$

$$\frac{1}{10^5} < {}_nC_3 \times 125 \times 10^{-12}$$

$${}_nC_3 > \frac{10^{12}}{125 \times 10^5} = 8 \times 10^4$$

$$n^3 > 6 \times 8 \times 10^4 = 48 \times 10^4$$

$$n > 78$$

Check

$$n = 78 \quad {}_{78}C_3 = \frac{78 \cdot 77 \cdot 76}{6} = 7{\cdot}6 \times 10^4$$

$$n = 79 \quad {}_{79}C_3 = \frac{79 \cdot 78 \cdot 77}{6} = 7{\cdot}9 \times 10^4$$

$$n = 80 \quad {}_{80}C_3 = \frac{80 \cdot 79 \cdot 73}{6} = 8{\cdot}2 \times 10^4$$

$$n = 80.$$

For two hours flight the significant number will reduce to 40, *i.e.* approximately inversely proportional to flight time.

APPENDIX II

EXAMPLE 1

Apparent safe life	10,000 hours
Frequency of vibration	10 per hour
Number of vibrations to failure	10^5
Vibratory stress, where f_L is fatigue limit stress	$2{\cdot}92\,f_L$
Unfactored measured stress	$= \frac{2{\cdot}92\,f_L}{1{\cdot}2} = 2{\cdot}43\,f_L$

First Approximation

From Fig. 7 factor required	$= 1{\cdot}58$
Factored stress	$= 1{\cdot}58 \times 2{\cdot}43\,f_L = 3{\cdot}85\,f_L$
N to failure (mean curve)	$= 4{\cdot}2 \times 10^4$
Safe life	= 4,200 hours

Second Approximation

p required	$= 4{,}200 \times 10^7 = 4{\cdot}2 \times 10^{-4}$
x from Tables	$= 3{\cdot}34$
Factor required	= Antilog (0·0144 + 3·34 × 0·012 × 4·624)
	= Antilog (0·0144 + 0·185)
	= Antilog 0·1994
	= 1·58 N.E.

EXAMPLE 2

Apparent safe life	= 10,000 hours
Frequency of vibration is 250 r.p.m.	= 15,000 per hour
N to failure	$= 15.000 \times 10{,}000 = 1{\cdot}5 \times 10^8$
Design stress	$= 1{\cdot}005\,f_L$
Maximum measured stress	$= \frac{1{\cdot}005\,f_L}{1{\cdot}2} = 0{\cdot}838\,f_L$

from Fig. 7 using Curve *B*

N to failure	$= 6 \times 10^5$
Reliable safe life	= 40 hours.

Probability of Failure at 10,000 *hours*

Factor = 1·2

Apply distribution formula

$$\log F = \log \bar{X} + x \,.\, \sigma$$
$$\log 1{\cdot}2 = 0{\cdot}0144 + x \times 0{\cdot}012 \times \log (1{\cdot}5 \times 10^8)$$
$$0{\cdot}0792 = 0{\cdot}0144 + 8{\cdot}176 \times 0{\cdot}012x$$
$$0{\cdot}098x = 0{\cdot}0792 - 0{\cdot}0144 = 0{\cdot}0648$$
$$x = \frac{0{\cdot}0648}{0{\cdot}098} = 0{\cdot}661$$

From probability tables

$$p = 0{\cdot}25$$

i.e. probability of failure is 1/4 which is unacceptable.

Reduce life to 2,500 hours

N to retirement $= 3{\cdot}75 \times 10^7$ log = 7·574

therefore $7{\cdot}574 \times 0{\cdot}012x = 0{\cdot}0648$

$$x = \frac{0{\cdot}0648}{0{\cdot}091} = 0{\cdot}714.$$

Probability of failure p = 0·235 *i.e.* virtually unaltered.

DISCUSSION

C. J. Costain (D. Napier & Son Ltd., Acton): Mr. Le Sueur's excellent lecture showed that the Air Registration Board had evolved the philosophy on which to base their new requirements in time for the first British gas turbine engined rotorcraft to be certificated for civil use.

He hesitated to comment on Mr. Le Sueur's conclusions on engine failure probability. Even now, after several years of development experience and some service experience of turbine-engined rotorcraft, total flying time amounted to only a few thousand hours, and statistics based on this experience were of little value. But it could be said that nobody had been hurt in one of these rotorcraft.

A word on Contingency ratings. The task of the engine manufacturer was to produce a set of ratings, economically attractive to the rotorcraft manufacturer and operator, but not pitched so high that it was next to impossible to get through the mandatory 150 hour type approval test. Napiers had now a good deal of experience in this line. A specific example was the Gazelle Mark 500 in the Westland Belvedere. This twin-engined rotorcraft required between 1,800 and 2,000 s.h.p. for take-off at normal all-up weight. The engine was rated at 1,200 s.h.p. for Maximum Continuous, 1,465 for Intermediate Contingency and 1,650 for Maximum Contingency. The latter two ratings were for one engine out use only. Thus Intermediate Contingency produced about 70–75 per cent of the normal twin-engined take-off power and was adequate to get the aircraft home on one engine. Maximum Contingency was equivalent to 82 per cent of the take-off power, which was sufficiently high a percentage to allow the pilot either to cushion his descent or to lose minimum height in gaining forward speed following engine failure near the ground.

From the known life of a turbine blade to creep rupture and the level of powers at which the engine ran before failure they could establish turbine blade operating temperatures.

The present A.R.B. multi-engined type test called for 1½ hours at Maximum Contingency, 26 hours at Intermediate Contingency and 56 hours at Maximum Continuous during the 150 hour type test. Using current blade materials, about 15 per cent of the total blade life was consumed by the Maximum Contingency running, a very high rate of consumption, 20 per cent by the Intermediate Contingency and less than one per cent by the Maximum Continuous running. These percentages rose rapidly with only small changes in turbine entry temperature. However, at present temperature levels, and taking creep rupture failures only, the engine should have a good chance of completing three successive type tests or equivalent running in service.

If they remembered that neither Contingency rating would be used except as prescribed by Mr. Le Sueur's statistics their effect on engine overhaul life could be seen to be negligible.

There was however, good reason to log their usage, particularly if they were likely to be used a number of times during an engine's life, *e.g.* for crew training.

D. Hill (Chief Stressman, Westland Aircraft Ltd., *Associate Fellow*): With regard to engine failures it should be remembered that quite a large proportion of helicopter flying took place at high power in the hover over water and what might be a minor incident over land might become a major accident over water. Free wheels could be a source of unreliability and every effort must be made to make them foolproof or consider an alternative. Had Mr. Le Sueur considered the possibility of eliminating free wheels when engines with free output turbines were used?

A large number of helicopter roles took place close to the ground or sea and therefore if multi-engined layouts were considered as a safety measure it was essential that in the event of an engine failure, additional power must be rapidly and automatically available from the others. Due to redistribution of load it might be necessary to include the effect of "one-engine-out" for life substantiation of the transmission. Had the lecturer considered a suitable percentage time with "one-engine-out" to be included in the flight spectrum?

A gust acceleration of $+1{\cdot}0g$ quoted by the lecturer seemed rather large. More gust measurements on helicopters should be taken in this country to build up a realistic flight spectrum for fatigue substantiation. Now that the expected lives of helicopter components were becoming quite large, loads imposed "once per flight" should be considered. During every flight an acceleration of $1{\cdot}0g$ was exceeded; had Mr. Le Sueur considered a suitable flight spectrum to cover this case?

It seemed that the lecturer considered that the accident rate could be reduced by using increased safety factors. Mr. Le Sueur made the point that "Factors of Safety must achieve the desired result." He thought that existing factors had been demonstrated to achieve the desired result since he suspected that few of the accidents recorded could be attributed to the failure of components which had been substantiated using present factors of safety.

Was it reasonable to assume the same scatter in strength on a series of components produced to the same drawing by the same manufacturer and tested on the same fatigue testing machine as was found from the widely differing lug designs from different manufacturers which were plotted in Mr. Heywood's report?

When a normal distribution was assumed for a series of test results it was assumed that fatigue failures could occur at extremely low stress levels. Was this conception entirely satisfactory from an engineering point of view, bearing in mind the amount of inspection and control such as crack-detection, X-ray, finish and dimensional control carried out on the components before their installation in the helicopter? It would be difficult to establish safety factors of 2 to 3 by fatigue testing without imposing completely unrepresentative deflections, especially for such components as gearboxes.

Mr. Le Sueur: The necessity for a free wheel was basically that the rotor system and engine system or transmission should not interfere with each other. If the engine manufacturer were prepared to allow his free turbine to rotate with the transmission when the engine failed, then the necessity for a free wheel was probably eliminated.

Mr. Hill mentioned hovering close to the sea. In public transport this was not likely to arise. Helicopters in public transport would normally fly at least 1,000 ft. above local terrain and only descend to land. If the heliport were close to water, then the operation could be so scheduled that a safe landing might be made onto comparatively calm sheltered waters in the event of engine failure.

The one-engine-out effect on fatigue should be considered, particularly if the stresses involved were damaging from the fatigue point of view. The percentage of flight time to be considered under these contingency conditions could only be determined by flight statistics which at present were lacking. A figure of 10 per cent could be suggested as an initial guess.

The effect of the once-per-flight cycle of stress on the safe fatigue life of the rotorcraft components could be established in the strain gauging programme required before certification. The frequency of this cycle was a function of the probable role of the rotorcraft. An average of six flights per hour had been suggested, however, for any particular application; this figure was a matter for agreement between the applicant for a certificate of airworthiness and the authority.

The envelope conditions of $3g$ were to be applied to those parts of the structure that hung from the rotor rather than the actual rotor itself. The basis for design of the rotor system was fatigue rather than static loads.

Mr. Hill was quite right; they did want more information on the effects of gusts, they wanted to know the atmosphere better from the gust point of view, they wanted particularly, knowledge of the effect of high buildings and thermal effects, due to hot air rising from houses and industrial sites, for rotorcraft operating from city centres.

Mr. Hill had suggested that his examination of Dr. Heywood's report was not a fair one. In considering statistics it was necessary to cover as wide an envelope as possible

but it would be unfair to compare unlike quantities. In Dr. Heywood's report there were sufficient like quantities to make something of a statistical analysis. The method used was as follows:

Examining Tables V and VII, Dr. Heywood's report would show the following numbers of specimens.

TABLE V

Design No.	*Ultimate Stress p.s.i.*	*Mean Stress p.s.i.*	*No. of Specimens*
6	85,000	16,300	12
11	78,400	16,000	15

TABLE VII
Design No. 9 Ultimate Stress 71,700 p.s.i.

Mean Stress p.s.i.	*Machine*	*Location*	*Number*
0	Losenhausen	C. of A.	11
5,610	,,	,,	14
11,200	,,	,,	9
5,610	Schenck Pulsator	M.E.R.O.	15

Each of these six groups must be examined independently of each other.

In the determination of safety factor the prime consideration was scatter of results on like specimens tested under similar conditions, *i.e.* having eliminated all the accountable differences the safety factor was to be used to overcome the unaccountable differences, such as variation in material properties within the same specification, variations in workmanship on parts made to the same drawings, variations in the characteristics of the same testing machine, etc. This last was a difficulty not easy to overcome. However, if the analysis of all the results approximated to similar safety factors then something had been achieved with one overall limitation, *i.e.* the probability that all fatigue testing machines affected the results more adversely the larger the number of cycles to failure.

What drew his attention to the need for further examination of the results in Dr. Heywood's report was the apparent inadequacy of Dr. Heywood's ± 33 per cent variation curves, *e.g.* in Fig. 7 of his report there were a number of circled dots above and below Dr. Heywood's curves giving an overall scatter of best divided by worst of over 3·0 relative to the mean curve. This apparently called for a factor of 1·75 based on the mean result. As factors of only 1·2 had been considered sufficient to cover these effects it was necessary to bring the matter into the open.

Having plotted like points on log graph paper the best mean curve through the points could be drawn. Then, considering those points clustered around the several values of N equals 10^4, 3×10^4, 10^5, 3×10^5, 10^6, 3×10^6, the scatter, assuming a geometric mean, for each of these values of N could be determined. The value of the scatter increased with N and for all the specimens the approximate standard deviation was 0·012 log N over the range N equals $10^{4.5}$ to N equals $10^{6.5}$. Assuming a Gaussian distribution the number of standard deviations required to ensure a reliability greater than 1 over 10^7 was 5·2, and for 1 over 10^3 was 3·1. This information determined the bold part of the curves *A* and *C* in Fig. 7 of his Paper. The dotted parts were extrapolations of this information to reasonable limits.

There were no doubt flaws in this argument, but it was put forward in the hope that it would encourage further research into the problem of fatigue safety factors and as a first attempt for determining what safety factors were necessary. To test 1,000 h.p. gear box at 3,000 h.p. might be totally unrepresentative. Information was required on scatter; once the information was available agreement could be reached on what test factors were necessary. In discussion he did put forward the suggestion that maybe the gear box manufacturer or the helicopter manufacturer might be prepared to test every gear box that he manufactured beyond the fatigue limit. This would involve every gear box being tested for something like 55–70 hours. This problem was the concern of everyone involved and these figures were put forward to promote argument and to find the information necessary to make helicopters as safe a vehicle as they all—not just the Airworthiness Authority—but all, the manufacturer, the operator, and the general public would accept.

There was one point he thought that Mr. Hill had missed. In the paper it was assumed that zero stress would produce a failure. As he assumed a logarithmic distribution on stress, minus infinity occurred at zero stress, so therefore there was no zero stress in his argument at all. He appreciated this was a fallacy but at least it did reduce the factors. If one took the same information and assumed a normal distribution of stress for a probability of failure of less than 1 in 10^7 it was necessary to assume zero stress at something like 5 million reversals which was just ridiculous. So a logarithmic distribution of stress was assumed to get the factors down to a reasonable level. This meant that if a specimen failed at 20,000 p.s.i. and another specimen at 5,000 p.s.i. for the same number of reversals, the mean value was 10,000 p.s.i. not 12,500 p.s.i. with an overall scatter factor of 4·0 and a minimum safety factor of 2·0 on the mean, not 2·5.

V. A. B. Rogers (Assistant Chief Stressman, Fairey Aviation Division, Westland Aircraft Ltd., *Assoc. Fellow*): Many of them, he thought, would remember that some time ago there was quite a lot of discussion on a J.A.C. Sub-Committee that was trying to establish the factors on stress or life that should be considered when assessing the fatigue strength of airframe structures for inclusion in Av.P970 and the subject discussed there was the same as they were discussing now: that was,"What is a reasonable factor on stress that one should apply to the mean test result?"

At the time it was suggested that the R.A.E. were quite happy to accept the probability of failure implied by fixing the bottom limit of the scatter on life to be three standard deviations below the mean. The magnitude of the standard deviation derived from test results on light alloy structures was such as to give coefficient of variation of stress of 10 per cent at a life of 10^6 cycles. This resulted in factors on stress of 1·43 on the mean result of a large number of specimens and 1·56 on the result from one specimen. (The corresponding life factors were 3·33 and 5·0 respectively and these had in fact been incorporated in Av.P970.) This, he would suggest, was something they ought to think of in terms of Mr. Le Sueur's factors which were now ranging between 1·5 and 3·0.

At the J.A.C. discussion he was considered as "the odd man out" when he suggested that since they were talking about an *S-N* curve and not about helicopters or airframes as such, one should have a consistent philosophy for determining the factors that one should apply over the entire range of the *S-N* curve to achieve the desired probability of safety. At this time, when they were talking about factors between $1\frac{1}{2}$ and 3, ought consideration to be given to the arguments put forward previously so that one could get a consistent argument for the factor that one should apply? He thought the factor finally chosen would depend on what the coefficient of variation was and he thought this was really Mr. Hill's point. Mr. Le Sueur, by taking Heywood's *S-N* curves had stirred them all up a little because he thought they all felt that the scatter on those *S-N* curves was rather severe as, although he felt sure Mr. Le Sueur had tried to be fair in his representation, they all knew that lugs were funny things. He knew that structures in general were as well, as far as fatigue was concerned but, one got very different answers from lugs of comparable area, but, of different shape and thus he thought they had to be careful in making generalisation from lugs.

Obviously, Mr. Le Sueur wanted data from which these factors could be determined. It was up to them to provide this data, but, so that they should do so, he thought it would be far better if a definite probability of failure was defined. A coefficient of variation could then be established from this data and this would give them a rational basis for determining their factors. Not so long ago, they had a situation

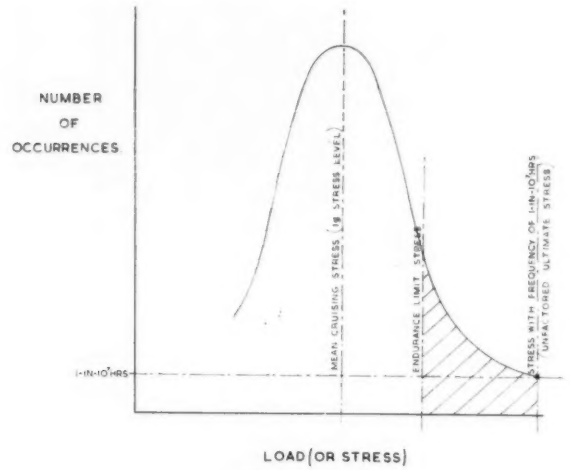

FIGURE A.

when the airframe people were asking for 1·56 on stress and saying "We don't know how you helicopter people get away with 1·2 on stress." Now, here was Mr. Le Sueur saying "I want 3 on stress," so they were bobbing up and down and he expected the airframe people would soon say "Lads, you need 5" and they would never build an aeroplane able to take off!

His second point was about the life that one determined for a component. At the beginning of Mr. Le Sueur's section on fatigue he referred to substantiation under the most severe applied loads. Now one had to be quite clear as to what one meant by the most severe applied load on a rotor. If they plotted a frequency diagram of load or stress measured on a typical rotor component they obtained a curve of the shape shown (Fig. A). They would like to be able to calculate ultimate conditions, fatigue conditions and so forth and establish the stresses associated with the present ultimate requirements of $3g$ and with the most frequently occurring condition of $1g$. However, generally they did not really know what the high g stresses were in a rotor component. Now Mr. Le Sueur had suggested that they ought to think in terms of 66 ft./sec. gusts. He thought they all would, if they knew how to calculate the stresses that occurred with a 10 ft./sec. gust, but, they could not do this satisfactorily. However, they must have some basis on which to design if they were to obtain Airworthiness Certification.

Mr. Le Sueur had said, and he agreed, that the strength of a rotor component was based on the fatigue argument. Now, referring to Fig. A again they could think of the most frequently occurring stress in a rotor component as the $1g$ stress level, and they knew what it was from flight measurements. They did not know what the ultimate stress level was and, for long-life helicopters, what the fatigue damage was from infrequently occurring stress levels. (Shaded area in Fig. A.) However, if they defined the unfactored ultimate stress as that stress which had a frequency of occurrence of 1 in 10^7 hrs. (a figure recently given to him by Mr. Le Sueur) and assumed that this stress was $2\frac{1}{2}$ times the "$1g$ stress level," then if they fixed the endurance limit stress level (*see* also Fig. A) at $1\frac{1}{2}$ times the "$1g$ stress level," the cumulative fatigue damage done in 10,000 hours of flight (coming entirely from stresses in the shaded area of Fig. A) was about 2·0. This showed that for a 10,000 hours life, which was a typical design aim for rotor components, it was the rarely-occurring high stresses that were significant.

He had suggested some time ago that to obtain a long fatigue life one should aim at having an endurance limit to $1g$ cruise stress ratio of 2 and that might be borne out by more investigation of the sort he had outlined. He thought that the ratio of $2\frac{1}{2}$ to 1 of unfactored ultimate stress to $1g$ stress level was too low and should be more in the order of 3 or 4 and this would raise the required endurance limit to about twice the $1g$ stress level. This was important because it set the scene as to the level one should use in establishing a fatigue life. If one took the most severe applied stress that could occur then this would be the ultimate stress, but he was sure Mr. Le Sueur did not intend that. Conversely if one used the $1g$ stress level there would be lots of early failures. They must establish where the endurance limit stress level should really lie and this required the derivation of the frequency curve of stress from actual flight tests rather than trying to determine what gusts and g levels they were going to meet. He felt that the more information of this kind was obtained, the nearer they would come to having long life helicopters.

Mr. Le Sueur: In an airframe, perhaps not so much in a rotorcraft as fatigue was probably the design basis for the rotorcraft, but in an aeroplane he believed it could be stated categorically that the spar structure sustained fatigue damage every time it took off. The factor of 1·56, he would submit, did coincide with the 1·5 that he quoted for 10^4 reversals in 10^4 hours, that was, one flight per hour. It looked as though the factors did agree, because he assumed the same figure of 10^{-3} at 10,000 hours; in other words he assumed 3 standard deviations. This lead him to believe that when one was considering higher frequencies higher factors were required.

Referring to Mr. Rogers' Fig. A—if they considered curve B of Fig. 7, which allowed for depreciation in reliability with extended life, it would appear that they could use this curve to factor the diagram in Fig. A. In other words the low frequency stresses required the low factors and the high frequency stresses high factors; and this might well be the answer to the problem of establishing a safe fatigue life on a sound basis.

F. S. Wood (Structures Dept., Royal Aircraft Establishment, *Associate Fellow*): His own concern had been with military helicopters, but with much of what Mr. Le Sueur had said he agreed in principle, although on some details they differed.

It appeared that for civil operations the Air Registration Board was considering a helicopter not as it was being operated today but as a vertical take-off air liner that used a helicopter rotor as a means of achieving powered lift. Therefore the spectrum of operational loading actions would be different from that on a military machine, and that would affect their views on airworthiness and the methods they used for proving it.

The accident figures given by Mr. Le Sueur did not indicate the period of time over which they were obtained. He believed that the general trend of the causes of helicopter accidents had changed in recent years; in particular the percentage of accidents attributable to mechanical breakdown was much less now than it was five years ago, and less still than it was ten years ago. He wondered how this trend would be reflected in the figures quoted.

Would Mr. Le Sueur indicate how many of the accidents attributed to mechanical defects were due to actual breakages —fatigue failures—of rotating mechanical or structural parts, and how many were due to breakdowns in the lubrication system, wear of bearings, and so on?

Mr. Le Sueur appeared to take for granted the inherent feature of the helicopter that unlike most aeroplanes it was capable of making a controlled descent and emergency landing remote from an airfield in the event of complete failure of the engines, usually without injury to the occupants. Admittedly their experience had been on small helicopters, and there would be a difference in outlook when they considered large helicopters carrying, say, 50 or more passengers. Nevertheless they had an extra factor of safety there which might be worth some consideration.

He questioned the proposal not to consider more than one engine failure at take-off until more than 42 engines were fitted. He suggested tentatively that they needed to keep their aircraft at least as safe as the typical four-engined aeroplane on which they considered one engine failure. Therefore on every machine with more than four engines one should cater for failure of two of them.

It would be interesting to know how the suggested take-off

requirements compared with the proposals for helicopter airports given in the I.C.A.O. Aerodrome Manual Part 10.

He thought that the fatigue strength factors Mr. Le Sueur discussed should be based on proper fatigue tests on representative helicopter components, in the same way that tests were being made at present. He would suggest that the standard of manufacturing finish on most helicopter rotating parts was at present so much better than that represented by the light alloy lugs quoted in the lecture that the high factors suggested were probably unnecessary. On the other hand the helicopter designer had to ensure that his standards remained sufficiently high, so that what were now regarded as acceptable scatter factors were in fact adequate for the substantiation of reliable fatigue properties.

Mr. Le Sueur: With regard to the accident analysis, there was a lot of information missing but accident analysis was so difficult to describe concisely. The 639 accidents in general operation covered the period 1942-1959; whereas those 151 major accidents for which flying hours were known only covered the period 1949-1959, and then not completely. Further breakdown of the accidents and their causes in detail was considered to be outside the limits of his paper. However, Mr. Wood was quite correct when he stated that the accident rate was improving, particularly those accidents due to crew error. But the airworthiness accident rate was still higher than could be accepted, particularly when considering scheduled operation, for which the figures quoted covered the period 1956-1959.

As this paper related to Public Transport rotorcraft the subject of "General Purpose Rotorcraft" was not discussed, but Aerial Work rotorcraft would need as much consideration as the Public Transport machine, if not more.

As the Air Navigation Order, Regulations and Requirements were at present written it would appear that the standard of airworthiness required for Public Transport aircraft was higher than that required for Aerial Work. In the case of the rotorcraft crane, it was possible that the reverse might be necessary.

The preservation of human life was not the entire reason for airworthiness; economic factors must also be considered, including insurance rates on the actual machine and other people's property.

Engine Failure during Take-off. In Appendix I it would be seen that second engine failure during take-off was only significant on a twin if the engine failure rate was higher than 1 in 300 take-offs.

Mr. Wood said that the making of the parts of a helicopter transmission involved such a high degree of quality control that the scatter factor was bound to be lower than it was for a lug made in the ordinary workshop. But what of the clumsy worker who, working next to the helicopter, moving a ladder, or vehicle or taking the weather proof covers off the blades, accidently knocked a vital part of the helicopter without knowing it; what of the fitter in the shop who accidently ran a file across the edge of the beautifully polished surface of a particularly vital piece of transmission? A transport vehicle had to be robust, and a little less finnicky quality control and a little more scatter seemed to be a safer way to design a helicopter than very fine limits, very smooth surfaces, and very small factors. Paradoxically it might be that there was less scatter and hence lower safety factors with less quality control, albeit the mean value would be lower.

J. S. Shapiro (Director, Servotec Ltd., *Associate Fellow*): He was under the impression that an aeroplane airworthiness standard for engines was nearer one failure in 5,000 hours of operation than one in 2,000 hours. He wondered whether there had been a deterioration in the standard of reliability in recent years. Also, what was the definition of a major and a minor accident?

The main point he wanted to make was that Mr. Le Sueur was, he believed, trying to put several standards in the same category. A helicopter had a large number of parts. Nobody, not even Westlands, could design and test every part with the same degree of thoroughness. The majority of components must be designed so that with the minimum effort of analysis and development they remained safe. For such components, he thought that Mr. Le Sueur was right and they must have a multiplying factor of three. In fact, he had a rule of thumb, namely, take the ultimate strength and divide by 15 and you were pretty safe. In many such cases, there was not even a sacrifice in weight because these components were designed by considerations other than fatigue strength.

However, there were a number of parts where one just could not follow this procedure but must examine the matter more thoroughly, and it was wrong to seek general rules. Referring particularly to lugs, he thought that many lug elements in helicopters were very different from lugs in aeroplane structures. Structural lugs were often badly designed. It was not even possible to speak of stress concentration which described a stress peak in a microscopic region. Some lugs had a badly distributed stress pattern and the maximum stress prevailing over quite a broad region could be as much as four times the nominal stress. The actual value was affected by the fit between the lug and the pin and other points of precise geometry which could be looked after at several critical spots of a helicopter. For this reason, scatter values derived from experiments with indifferent structural lugs could not be a guide for designing critical lugs in helicopter mechanisms.

He could see some backing for this point of view in the fact that the probability of accidental failures appeared to be four times greater in unscheduled operations than in scheduled operations of helicopters.

One should not ignore the fact that pure fatigue loading hardly ever occurred in typical helicopter components as it did in many aeroplane components. In helicopter components one had the combination of fatigue and wear or fatigue and corrosion, both of which were subject to many factors which could increase the scatter of results. In short, he believed that the type of approach to designing for fatigue strength as they had heard was very useful to the designer when he dealt with the 95 per cent of components which he could not examine thoroughly. When the designer came to the 5 per cent of components which he could and must analyse and develop thoroughly, an entirely different approach was required.

Mr. Le Sueur: The usual definition was used for minor and major accidents:—A major accident was one which caused serious or fatal injury to the occupants or serious damage to the aircraft, a minor accident was one which caused only minor injury or minor damage. There were at least two reasons why the major accident rate for general operation due to mechanical defects was 1·3 per 10,000 hours, as compared with 0·3 per 10,000 hours for scheduled operation, (*a*) the type of helicopter used in general operation was the much earlier form of helicopter. Not so much was known about fatigue in the design stage. A large number of the failures were in the tail rotor transmission. (*b*) The different type of operation. General operation included crew training, and aerial work; the pilot in aerial work was likely to handle the machine more roughly than in scheduled operations. In fair weather scheduled operations the likelihood of fatigue damage due to manoeuvres was a lot less than that in aerial work or crew training. In the designing of joints much use had been made of force fits and expansion joints. In examining another report on the fatigue properties of joints using force fits, apart from the significant increase in allowable stress for the same number of reversals, was the increase in scatter on the results. With only four results the *S-N* curve was a veritable "donkey's hind leg." It would therefore appear that high safety factors were as necessary to cover the scatter on controlled force fits as on the more usual push fit or clearance fit joints.

D. M. Jameson (Air Registration Board, *Assoc. Fellow*): There was one point on the question of philosophy: on the curves *B* and *C* on Fig. 7 there was an assumption, as he understood it that one could tolerate a probability of one in a 1,000, of a catastrophic accident on the last flight on which

one dispatched the helicopter, this probability being based on the situation as one knew it just before take-off. It seemed to him that they must be concerned with not the average possibility of an accident over a long series of operations but over a probability they would accept on the worst flights. If they looked after the worst flights, the average would look after itself. This was possibly quite a distinct philosophy from a military philosophy when one might say one could afford to lose perhaps x per cent of the machines in order to achieve some vital objective. Therefore, looking at the curves *A*, *B* and *C*, he was quite happy with curve *A* because it was based on a constant probability of failure but he was not happy with the other two curves, where it seemed to him that Mr. Le Sueur was being far too lenient towards civil transports in the allowable risk contemplated for the worst flight.

A. F. Chalmers (Westland Aircraft Ltd., Fairey Aviation Division, *Graduate*): He would like to comment on the establishment of the *S-N* curve. He gathered from the stress factors that Mr. Le Sueur presented on Fig. 7 that they had one point of agreement with those established by the J.A.C. Sub-Committee referred to by Mr. Rogers. The 1·56 factor quoted by Mr. Rogers for one specimen appeared to agree with one end of curve *C* on Fig. 7.

He understood from Mr. Le Sueur's argument that his factors must also apply to only one specimen, so he concluded that Mr. Le Sueur's mean *S-N* curve assumed only one specimen tested at each stress level. If that were so, then they should be able to claim some reduction on Mr. Le Sueur's factors by testing several specimens of the component to be cleared at each stress level. This would give a greater confidence in the mean *S-N* curve and would result in lower stress factors for the same probability of failure.

Mr. Le Sueur: He felt that if the Industry could supply them with the information which justified less factors they could accept them. The curves which had to be factored were the mean curves determined from a number of results. If the aircraft manufacturer were prepared to test a sufficient number of specimens to determine his own safety factors there was no objection, in fact the opposite.

W. Tye (Chief Technical Officer, Air Registration Board, *Fellow*): He would like to go back once more to Mr. Heywood's famous lugs. It seemed to him quite extraordinary, and quite deplorable, that they were still arguing about scatter when it was of such fundamental importance. He remembered saying much the same sort of thing that he was about to say some eight years ago after the first crop of fixed-wing aircraft fatigue troubles. It then became clear that as soon as one began using statistical principles to interpret the very small amount of data available on the distribution of life, quite gigantic factors were necessary if one was going to work to the "10^{-7} level" of safety. He supposed largely because of this, the fixed-wing people had largely abandoned the safe-life concept altogether and gone over to fail-safe methods, which was a much more respectable approach.

He did not see the helicopter design lending itself readily to such an approach, but if that were the case, then it did mean that one would have to face either very high factors of the sort described, or else much more exact knowledge of the scatter shape which might admit some reduction of factors. He had always had misgivings that the shape of the scatter which one had to assume as a normal distribution in the absence of better data, might be false when one was going to the extreme tails of the distribution curves. The test data available was usually clustered around the middle of the distribution curve, so clearly one had no great confidence in the extrapolation necessary. Thus, they came back to the need for a gigantic effort in getting properties of scatter from a very large number of specimens. In fact, the plea he made for fixed-wing aeroplanes eight years ago, he re-made now.

R. E. Hardingham (Secretary and Chief Executive, Air Registration Board, *Fellow*): He would like to try and lift a little of the depression that Mr. Le Sueur's Lecture might have caused the helicopter Industry. The Air Registration Board were a cunning lot, they often adopted a policy in framing new requirements which aimed at improving the breed. Some years ago the Board was dissatisfied with the performance of wheel brakes; and devised requirements which were thought to be impossible to meet. However, in due course, disc brakes appeared and the requirements were met.

In the early days of helicopters when there were no airworthiness requirements, the A.R.B. persuaded Professor Bennett, Mr. Shapiro, Mr. Hafner and a number of others to sit round a table and they offered them a set of provisional requirements. Being the honest British citizens that they were, they eventually devised requirements that were much tougher than those the A.R.B. had in mind.

Those were examples of A.R.B. policy and possibly Mr. Le Sueur's lecture was another example where they in the A.R.B. had come in force to listen and take note of their reaction to a number of tough proposals.

In fairness to the A.R.B. and Mr. Le Sueur, he suggested the discussion had shown a general lack of information in certain directions and this they must find a way to overcome.

W/Cdr. R. A. C. Brie (Assistant to the Technical Director, Westland Aircraft Ltd., *Associate Fellow*): *Contributed*. Authoritative opinions on helicopter take-off performance requirements such as had been expressed by the lecturer had a special significance in relation to heliport size. This must be dimensionally adequate to meet foreseeable operational standards, but in any densely populated area there were economic and practical limits to what could be achieved. Hence it was the responsibility of the designer to ensure that predicted performance estimates for any particular type of helicopter were capable of reproduction by an experienced pilot within a given framework of operational conditions. This precluded unconventional departures from accepted standards of pilotage, and he was surprised that the lecturer should have thought it worthwhile to comment on the accentuated vertical or near vertical take-off conception which he believed to be unduly hazardous as a manoeuvre, mechanically extravagant in component life, and economically unacceptable in waste of time.

In any event it must be assumed that normally a central city heliport would be completely surrounded by comparatively low level obstructions, and that omni-directional take-off and approach paths of uniform obstruction-free gradient angle would be obligatory to facilitate operation under zero zero conditions of horizontal visibility. Hence he would welcome some further explanation of the site depicted in Fig. 5 which, with a major obstacle on one side only, appeared to be of illogical and unrealistic design. With the take-off in the reverse direction to that indicated it would appear that helicopter operations would be impracticable.

It would also be helpful to know to what extent the dimensional values of 'O' to boundary in each of the two illustrations compared with the 400 ft. for unobstructed conventional take-off which experience indicated was a reasonable distance to safeguard the one engine inoperative emergency condition.

Mr. Le Sueur: The vertical-backward take-off had not yet been proved. It was hoped to conduct experiments exploring the practicability of such a technique when the distances required would be determined. Fig. 5 was only illustrative and the possibility of omni-directional take-offs and landings was not intended to be ruled out.

The main reason why he considered the take-off described in the lower half of Fig. 5 safer was (*a*) with the quasi-vertical-backward take-off, the pilot was apparently flying in a direction in which he could not see; (*b*) he was also flying at a very low speed to a high altitude without, at the present, adequate instrumentation. With the conventional forward take-off to a low height and accelerating along the ground, he was quickly reaching a condition where the power required was smaller than it would be in the vertical climb, the rotorcraft was achieving maximum benefit from the ground cushion and the pilot was always looking where he was going.

The Royal Aeronautical Society
The First Fifty Years

BY

J. LAURENCE PRITCHARD, C.B.E., Hon.F.R.Ae.S., Hon.F.I.A.S.

Since his retirement Captain Pritchard has been doing much research in aeronautical history which has thrown new light on the Society's history and the work of its members, but there are still many gaps to be filled. It is hoped that the publication of these abstracts from the history of the Society which he is writing may encourage members to contribute to the history from their own knowledge. Much of aviation history may still be hidden in family archives.

1874

Thomas Moy's declaration at the 1873 Meeting of the Society, that he and his partner Shill were constructing a large-scale model of a flying machine, roused considerable interest. Twelve months later, at the Meeting held on 15th May 1874, he said:

"We have not got on so fast as we had hoped. Various difficulties had arisen which had to be mastered one by one as they cropped up. As their machine was only a working model, the aeroplane required to be made of some light material. Some time ago I was trying to get thin steel rolled and corrugated but I was unable to get anything suitable. I next tried a fine kind of brown holland for our wing sails, but found this would not take the curves kindly, but got into wrinkles. I am now making use of thin pine lathes nicely planed and fitted so as to form a sectional screw surface, and I think they would answer very well, and would form the nearest approach to a perfect screw that can be obtained."

Over eighty years after Moy had spoken, designers are still faced with difficulties and are trying new materials, even thin sheet steel, corrugated! It is a proof of the seriousness of their work that Moy and Shill found so many difficulties to progress. Too many inventors had come forward in the past with untried but complete imaginative solutions of the flying problem.

The frontispiece of the 1874 *Report* shows an engraving of Moy's and Shill's Aerial Steamer. A general description is given in the Concluding Remarks of the year. The steam engine weighed about 80 lb. and developed 3 h.p. according to the *Report*. Although tantalisingly enough, as with many of the statements about power in these early experiments, it does not give any idea of fuel, duration, water, or whether the boiler weight is included.

The engine drove two airscrews, each six foot in diameter and each containing six blades. The total area of the screws was 60 sq. feet, that is, five sq. feet for each blade. The screws revolved between the two lifting surfaces, a front one of 50 sq. feet and a rear one of 64 sq. feet. The total weight of the Aerial Steamer was 214 lb. (again fuel and water were not mentioned!) and Moy reckoned that it would take off at 35 m.p.h., following a series of experiments which he and Shill had carried out at Penn's Factory, Greenwich.

It was decided to try the machine in the open air, under control, at Crystal Palace. One of the fountains had a path round it of about 150 feet radius. A pole was erected in the middle of this fountain with cords from each end of the Aerial Steamer, to keep it at a uniform distance from the centre. Moy's machine had a tricycle undercarriage, but the track was so rough that the first attempt had to be quickly abandoned, for fear the structure would be shaken to pieces.

A wooden track was constructed and late in the year steam was raised for the great event. Alas for expectations! The maximum speed obtained was only 12 m.p.h. instead of the 33-35 m.p.h. necessary for lift, as calculated. The *Annual Report* stated, however, "This is the first time that a machine weighing two hundred pounds has ever been driven by its own motive power by revolving planes impinging on the air."

Moy was not only not disheartened, but was encouraged to test out the possibility by vertical flight with his "aeroplane wheels." A pair, 12 ft. in diameter, were fitted with planes covered with linen, and driven in a horizontal plane. But the construction was not strong enough and succeeding experiments failed to get the lift Moy had expected.

Some of the experiments were witnessed by the Duke of Argyll, the President of the Society, the Duke of Sutherland and Earl of Dufferin, Vice-Presidents and Brearey, the Honorary Secretary, who were satisfied that the 3 h.p. engine could supply enough power to the horizontal screws to lift considerably more than its own weight. This they believed was a worthwhile experiment, showing that they were on the right road. By further experiments it was hoped to increase the margin between lift and power weights. There is no doubt that hopes rose high towards the end of 1874, following the experiment.

Moy, during the meeting, mentioned that he had talked over, with Glaisher and Brearey, the possibility of raising and lowering balloons by means of horizontal airscrews and had hoped the Society would buy a balloon so that the experiments could be carried out. It was not the first time, nor was it to be the last, when inventors had suggested that the Society should subsidize their experimental ideas!

Charles Brooke, a member of the Society and a Fellow of the Royal Society, was present at the meeting, and it is of interest to note how the truly scientific mind dealt with the problem, as against Moy's practical engineer's method of "suck it and see." Brooke pointed out that all that was necessary was to have a rope attached to the centre of the airscrew and carried over a pulley. The lifting force could easily be measured by a self-registering apparatus which he undertook to make and present to the Society to use. Charles Brooke was well-known for his self-recording instrument work.

He took the opportunity to explain why the Society's experiments to find the pressures on flat planes had not given the results expected. The assumption had been made that the resistance was proportional to the square of the speed of the air. "It was supposed that the particles resisting the motion, as soon as they came in contact with the surface, were annihilated, and only fresh particles came in contact with the moving body. This is at variance with what must occur. The particles are compressed and had all to get out of the way sideways."

Brooke showed that the resistance must not only depend on the angle of incidence and the air speed, but also upon the way the plane was placed in the air stream.

The air was beginning to prove a very troublesome fluid, and the more it was thought to be understood and explained the more awkward it proved to be. Brooke was well aware that the problem was complicated by the compressibility of air, and Wenham, at the same meeting, expressed the opinion "that a very different law applied to an elastic medium like air than to water."

D. S. Brown, a member, had been experimenting with various forms of fixed wing models, with the object of increasing their stability. This was the fourth time he had lectured before the Society. His models consisted of fore and aft planes with the fuselage in between (tandem

monoplanes) and a movable tail and rudder in the rear. He suggested the main planes could be moved to provide a certain amount of thrust, or airscrews could be used in between the planes.

Modern gliding enthusiasts may be interested in the method of launching, which Brown suggested.

"Unlike a ship or wheel carriage, the bi-plane can only be supported by the air when it is in motion, as illustrated by a slate thrown upon smooth water, which is then sustained when moving horizontally without any inclination of its surface. It is therefore necessary that the apparatus should start with an initial motion, which may be given by an indiarubber rope fastened at one end to a post, and at the other, by means of a ring, to a vertical bolt inserted in the under part of the bi-plane. In stretching the indiarubber by drawing the machine backwards, it will afterwards spring forwards with any required velocity, at the same time releasing itself from the rope as the ring falls from the bolt when the rope slackens. With respect to the proportion of weight to surface, that will depend upon the velocity."

Brown quoted Sir George Cayley to back some of his arguments, and added, "It is desirable that the attention of country gentlemen should be attracted more to the subject, because not only have they the means and leisure, but, what is of the greatest importance, ample facilities in their parks for trying out the experiments on a large scale, by which alone mechanical flight can be rendered a reality."

I have often wished that a man could be transported in time, so that one could have a better historical criticism of those things which we say are progressive in any particular generation. There are not many parks left in England where aeronautical experiments could be carried out on a large scale, less than a century after Brown had spoken. Whether the urbanisation of the country is good or not, I would not like to say, without inventing many more terms of abuse and horror.

The lecturer declared what he thought were some of the advantages of flying when it came.

"It will combine the independence of a private carriage with the speed of an express train, and a person will often be able to arrive at the end of his journey by the time it now takes to start from the nearest railway station. Aided however by favourable aerial currents the voyage would sometimes be made in an incredibly short time, as now done by birds of passage. It would also be in a straight line and free from all obstruction. The motion would be of the most agreeable and least fatiguing kind, resembling skating, and in warm weather the aeronaut would be able to choose what temperature to travel in, which would depend upon elevation, whilst the expansive and ever changing view would be unrivalled."

Brown illustrated his lecture with a number of models to show his ideas on stability, to which he attached more importance than engine power, arguing that if the power failed, a stable aeroplane would glide down safely. Of his models launched during the lecture the *Annual Report* has only this to say, "All showed perfect stability, and, save one or two, floated in the air in a horizontal position across the room, a distance of between twenty and thirty feet, and, apparently in some instances would have gone further without falling had not the walls interfered."

It is a pity we know so little about these models, save that they were tandem monoplanes made of light wood and paper. In the *Annual Report* plan drawings are shown of two types, one with crescent-shaped planes and one with square planes.

The lecturer experimented with engine models as well as aircraft models. The engine was of no practical value but some of the material used was of considerable interest.

"I have here," said Brown, "a small aluminium tube 9 inches long and one inch in diameter. No doubt it will bear an integral pressure of 100 lb. Aluminium is four times as light as silver, and it can be rolled much thinner than copper without cracking like that metal. Unfortunately it cannot be soldered, but I have succeeded in uniting very thin pieces weighing half an ounce per square foot, by sewing them together and placing a leather or indiarubber washer between the united parts. This tube appears perfectly made, without a joint, but how I do not know. It weighs three-quarters of an ounce."

Brown said he paid 8s. an ounce for the aluminium he used, and Wenham stated that he could obtain it in bulk for 5s. an ounce. He thought £4 to £6 a lb. was dear. About 1850 the price was £51 a lb., but five years later it had dropped to £3 10s. a lb. It is of interest to note that in 1946, immediately after the Second World War, the price was quoted at £67 a ton, less than ½d. an ounce. The fundamental value of lightness for aircraft construction was more important in those days than now, when unlimited power at low lb./h.p. has enabled built-in resistances to be lifted into the air and helicopters to behave like aerial cranes, and flying bedsteads to keep one awake with their nightmares of sound.

In this *Report* was published another lengthy paper by James Armour entitled, "Resistance to falling planes on a path of translation." It was the subject of much experimental work by S. P. Langley, some twenty years later, the results of which were published in his *Experiments in Aerodynamics*.

James Armour was a member of the Institution of Civil Engineers. He had taken a considerable interest in the problem of flight and had, like many others with less mechanical knowledge and engineering experience, tried to formulate a theory of flight. The lack of experimental data to provide the necessary constants led him to make certain assumptions which could only be tested by experiment.

However, he based his arguments entirely on engineering principles and deduced that the power required for sustained flight might be small compared with that required to reach the "sustaining rate of flight," that is the cruising power. Armour was aware of some of the effects of compressibility, especially at high speeds and remarks in his paper, "as the co-efficient of resistance is found sensibly increasing with the velocity in high velocities, moderate velocities with the co-efficients relating thereto need alone be here considered."

T. J. Bennett, a member of the Society, who had translated, from the French, Marey's monumental lectures on *Flight in the Animal Kingdom* for the Society in the 1871 and 1872 *Annual Reports*, and M. P. Harting's paper on the same subject from the French in 1870, contributed an interesting paper on French aeronautical activities.

He began by outlining those of the French Aeronautical Society which had been founded in 1868 by Brearey's opposite number, Dr. Hureau de Villeneuve. The French Society was showing itself considerably more energetic about aerial navigation at that time than the British. *L'Aéronaute* was the official paper, "and it is the only instance in which a journal specially devoted to aeronautical science has been a continuous success." To say that after six years of a monthly publication in the late 1860's and early 1870's is a high tribute. No such paper would have succeeded in Great Britain or America at that time.

The French Society met twice a month to read and discuss papers on any subject touching on aeronautics. The Aeronautical Society and the French Society had two prominent members in common, Alphonse Pénaud and Dr. de Villeneuve. The latter had made a number of successful ornithopter models, while Pénaud had made fixed wing, flapping wing and ornithopter models.

The helicopter, in an elementary form, had been known from early times, and a number of toys and models incorporating the use of horizontal airscrews had been constructed before Pénaud had begun his experiments. A model, with two screws driven by a small steam engine, for example, was shown at the Society's Exhibition at the Crystal Palace in 1868 by another Frenchman, M. d'Amecourt.

Pénaud used tensioned rubber, instead of springs or model steam engines for his motive power. In 1870 he demonstrated before the French Aeronautical Society a contra-rotating twin-screw apparatus which gave a performance considerably in advance of previous models.

Pénaud gave his screws a highly polished surface and, to quote Bennett's translation:

"The inclination of the blades was only 3° to 5° from the horizontal. When started in a light wind it will rise slowly for 4 to 5 yards and then go off in a horizontal direction with increasing velocity for 60 or 70 yards. It then ascends rapidly, often disappearing in the distance, when in a few seconds it will reappear approaching its starting point at a height of 60 feet and with the rapidity of an arrow dash over the experimenter's head to the distance of 100 feet in the opposite direction. The total time occupied in this erratic flight being about 20 seconds. This toy is rather a dangerous one to fly, but the experimenter is well repaid for the risk run, by its marvellous flight, which demonstrates that a simple screw suffices for the support, the translation and the equilibrium."

Pénaud's great contribution towards the solution of the problem of flight, however, was in the fixed wing area. Many models had been made and flown with indiarubber and other forms of power, but none had been stable or could make a claim that they could be controlled in flight. Pénaud provided a fixed tail plane, set at a small negative angle to the main planes. The two-bladed airscrew was mounted behind the tail plane, making the model a pusher. Pénaud's chief object in adopting this position was to save damage to the screw if it struck an obstacle or made a bad landing. He demonstrated his ideas with a tractor screw as well.

Pénaud balanced his model, fore and aft, so that the centre of gravity was slightly in front of the centre of pressure of the main planes, giving the model a tendency to dive and so increase its speed. The tail plane angle was thereby decreased and the pressure on its upper surface increased, so always tending to right the machine.

The flight was undulating. Pénaud obtained lateral stability by turning up the tips of the planes, or by giving the whole plane a dihedral angle. With such a model Pénaud made flights up to 200 feet. As a result of his experiments he calculated that an aeroplane, built on the lines of his fixed wing model, would lift 85 lb. per h.p. He was far in advance of the experimenters of his time, but in the Secretary of the French Society, M. Hureau de Villeneuve, he found an enthusiastic supporter. In the ninth *Annual Report* much of their work was recorded for the benefit of other members.

Both men decided to study flapping wing flight by means of models. In Pénaud's model, reported by Bennett, the change in the inclination of the surface is obtained by the elasticity of the sail or back part of the wing, the little sprigs that support it being free to rotate round the rod that forms the front edge. Rubber springs run from the back inner edge of the wings to the centre of the rod which forms the main frame. These springs regulate the movements of the sprigs and give the wing its elasticity, performing a similar function to that of the hind claw of a bat. The torsion and changes in the inclination of the wings are thus regulated by the combined action of the pressure of the air and the springs. The front edge of the wing has a simple up-and-down movement, which the elastic motive power transmits to it through the intervention of a crank and two rods. When the wing is in its highest position at the end of the upstroke, the rubber springs before mentioned cause it to present the inferior surface forward at an inclination of 15°.

"From the descent of the wing, the resistance of the air causes the outer portion of the wing to twist into a screw shape, the back edge being higher than the front, and thus supports and propels. The inner portion of the wing always remains inclined up, and acts as a kite. In the upstroke the whole wing supports as a kite, its surface being inclined upwards, the back edge being lower than the front. The wing is thus divided into two distinct parts, one active and the other passive, the outer, which comprises two-thirds of the wing, both supporting and propelling, while the inner portion only supports. The machine is not altogether sustained during the upstroke, so that the down-stroke has to make up the deficiency."

The model flew a distance of 50 feet.

Alphonse Pénaud was a remarkable young man. He died in 1880, before his thirtieth birthday. In less than ten years, however, he had demonstrated three entirely different forms of flight and put forward a solution of the very difficult problem of stability.

Pénaud wrote a number of papers for *L'Aeronaute*, one of which, although unfinished, on Sailing Flight, discussed rising currents and the effect of ground irregularities on the movement of air currents. He also pointed out the effect of the sun's heat to produce rising currents.

In 1876 Pénaud joined forces with another enthusiast, M. Gauchot, to build a full scale aeroplane capable of carrying two men. Two points of construction are of interest. The machine was what would be called in these days a high wing monoplane. The fuselage was wholly below the wing, but the pilot's head was protected against the wind by a glass box. It was proposed to fit the machine with a retractable undercarriage!

Pénaud's ideas were ridiculed and he was unable to get any help, financially or otherwise, towards testing them out.

His opposite number in France, Hureau de Villeneuve, was as firmly convinced that if you wanted to fly, the only way was to flap your wings. He made many successful ornithopter models.

Chanute wrote of him, in his *Progress in Flying Machines*, "He has, from first to last, designed something like 300 experimental models, so that his garret is a complete aviary of artificial birds."

One model, powered by twisted rubber, was able to start directly from the ground.

The Society began the tenth year of its existence with a membership of 119 and £39 in the bank. There were members in England, Scotland and Wales, but none in Ireland; in the United States, France, Germany, Denmark, India and Australia given in the members' list for 1874. In less than ten years, in the years when transport was slow, posts often difficult and uncertain, the Society's membership had become worldwide.

But Brearey had reported to the March 1874 meeting of the Council that he had sent out 4355 circulars in an effort to increase the membership, and as a result only twenty new members had joined!

At the end of January 1875 Brearey wrote a remarkable letter to the *Daily Telegraph* which began:—

"The time, I think, is coming when the public interest in aeronautical efforts is likely to be intensified. Its students have been quietly marching to the front these nine or ten years past. One day they will take the world by surprise. There are two divisions. The banner of one is levity; the banner of the other is gravity. We must look upon levity with gravity, and we must not treat gravity with levity."

Brearey discussed, in general terms, the pros and cons of both lighter and heavier-than-air machines, and particularly drew the attention of the readers of the *Daily Telegraph* to the heavier-than-air experimenter.

"He possesses much encouragement from the various flying machines which have been for ages the constant source of his observation and study. It is no discouragement to him that those flying examples—the birds—are possessed of vitality which he cannot hope to imitate."

REFERENCES:—*Annual Reports* A.S.G.B., 1874; *L'Aeronaute*, 1874; O. Chanute, *Progress in Flying Machines; Daily Telegraph*, January 1875.

TECHNICAL NOTES

The Production of Peaked Velocity Profiles

P. G. MORGAN and A. SAUNDERS
(Department of Engineering, University of Manchester)

IN MANY internal flow problems, the velocity profile of the flow in a duct of a given cross section is of major importance. In combustion chamber flow, for example, the entry velocity profile is a critical factor in determining the relative amounts of primary and secondary combustion air. Further, in the flow in an axial compressor, the peaked velocity profile between the root and the tip of the rotor blades has a marked effect upon the design of the blading[1]. Recent work by Zanker[2] has shown the effect of variation in velocity profile upon the accuracy of orifice-plate flowmeters. The variation in velocity profile has been widely studied for turbulent flow, and its relation with Reynolds number for pipe flow and for the flow between parallel plates is well known. With many internal flow problems, however, profiles are much more peaked than those associated with turbulent flow, *i.e.* the peakiness has been artificially raised.

1. INTRODUCTION

It is difficult to define the velocity profile in a straightforward manner, since the exact shape of this profile is not necessarily a simple curve, as in the case of laminar flow or for the fully developed turbulent profile. There may also be a lack of symmetry about the axis of flow, which is also difficult to define, so that the simplest definition which can be used is the ratio V_{max}/V_{mean} for the profile.

To examine the effect of the velocity profile in different configurations, some means must be adopted to generate suitable profiles. This problem has been treated by Livesey *et al.*[3] in connection with gas turbine combustion chamber flow. They describe a method whereby peaked velocity profiles may be obtained in a circular duct with the ratio V_{max}/V_{mean} up to a value of 1·42, by using a specially constructed baffle. Ramsay and Winternitz[4] have described tests on the effect of inlet conditions on the performance of conical diffusers, with particular reference to the inlet velocity profile, and have confirmed the views of Robertson and Ross[5] that inlet conditions may be as important as diffuser angle in determining the flow in a diffuser. Their method of obtaining different inlet conditions was by retardation of the flow in the boundary layer region of the circular duct by means of a projecting screen ring and it is considered that this approach is the most suitable method for a systematic investigation of the screen geometries which produce a peaked velocity profile. This approach has also been suggested by the known effects of wire gauze screens upon the smoothing out of irregularities in the velocity profile in a duct.

2. TEST FACILITIES

The test equipment used in this work is similar to that described by Livesey[3]. It consists of a duct of 4 in. internal diameter and of length 40 ft. with air blown through it by a centrifugal fan. The screen which generates the velocity profile is mounted between two flanges in the duct, great care being taken to obtain axial symmetry of the central hole of the screen (Fig. 2). The

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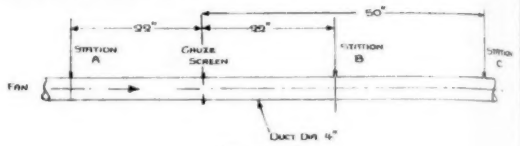

FIGURE 1.

gauze screens themselves are carefully cut by hand, with particular care taken to avoid any distortion of the wires.

For each test a series of static pressure readings were taken at the duct wall both upstream and downstream of the screen, and at the sections shown in Fig. 1. Velocity distributions were measured across two mutually perpendicular diameters. The mean of these velocity profiles was used in the analysis, and in most cases there was very little difference between the two. All static and total pressures were read on a bank of multi-tube water manometers.

Details of the gauze screen dimensions are given in Table I.

TABLE I

Screen No.	*Wire dia. in.*	*Mesh wires per in.*	*Open area ratio*
1	0·017	24	0·592
2	0·013	22	0·714
3	0·030	11·5	0·655

3. TEST PROGRAMME

Tests were made using the three gauzes detailed in Table I. The first gauze was tested as a complete screen across the duct, and the pressure drop measured at various velocities, to correlate the pressure drop coefficients with those obtained in previous investigations. The first central hole of smallest diameter, *i.e.* ½ in. was then cut, the screen being replaced between the flanges. The pressure drops were then measured again for the same rate of flow, together with the velocity profiles at sections *A*, *B* and *C*.

It was found that a coarse gauze mounted well upstream of the screen under test provided a much more uniform approach velocity profile, and damped out any irregularity in distribution across the duct due to the closeness of the fan to the working section.

Preliminary tests showed that the static pressure distributions along the duct were similar to those obtained

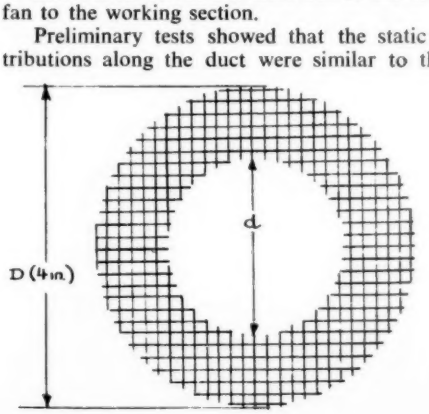

FIGURE 2.

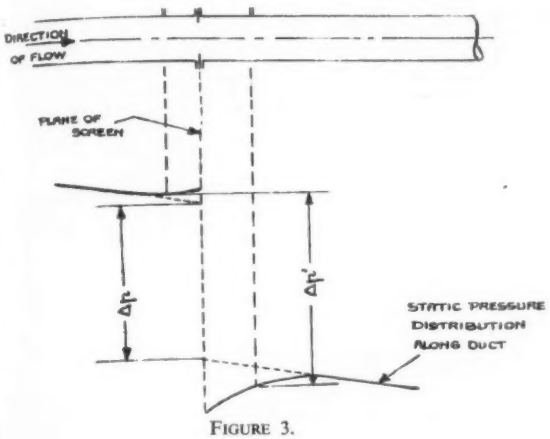

FIGURE 3.

by Livesey and did not reach the region of steady fall of pressure with distance until some distance downstream of the screen, *i.e.* the disturbance of the static wall pressures extended some distance up and down stream from the screen. To evaluate the pressure drop coefficient, therefore, it is desirable to extrapolate the lines representing the loss of pressure due to friction of the wall of the duct, and obtain a value of Δp as shown in Fig. 3. This method of measuring pressure drop is possibly open to criticism in this particular application on the grounds that the velocity profile changing mechanism is due to both the screen and the frictional effects of the pipe wall, and it might be argued that the pressure drop should be taken as the difference between the static pressures measured at two tappings, one upstream and one downstream of the screen, giving, for example, a value such as $\Delta p'$. The difficulty lies in deciding where the static tappings should be placed; in this work, therefore, the extrapolation method is preferred, particularly since the velocity profile production of the screen rather than the pressure drop characteristic is the main subject of the investigation.

The first position for the measurement of the velocity profile downstream was chosen to be in the region of steady static pressure fall. A second velocity profile measurement was made farther downstream at *C*, to examine the change in profile downstream of the screen; this has proved to be of great interest, and is the subject of further investigation.

The tests on a given gauze were repeated for a number of different diameters of the central hole, and the whole series then repeated for each different gauze.

The mean velocity at inlet lay in the region 109-150 ft./sec., comparable with Livesey's test conditions and corresponding to a Reynold's number, based on the duct diameter, of $2{\cdot}3$ to $3{\cdot}2 \times 10^5$.

4. PRESENTATION OF RESULTS

The first curves, Fig. 4 (*a*), (*b*) and (*c*) show the variation of V_{max}/V_{mean} with d/D. Each curve is plotted for a different screen. In each case, the inlet velocity profile was such that the variation of V_{max}/V_{mean} was within two per cent of 1·05, *i.e.* less than that corresponding to the turbulent velocity profile appropriate to the Reynolds number of the flow; this was due to the presence of the settling wire screen well upstream of the test section.

All the screens tested showed a maximum value of V_{max}/V_{mean} at a d/D value of approximately 0·5, and this is also shown in the cross plotting of Fig. 5. The extreme limits of d/D, *i.e.* 1 and 0, correspond to a completely

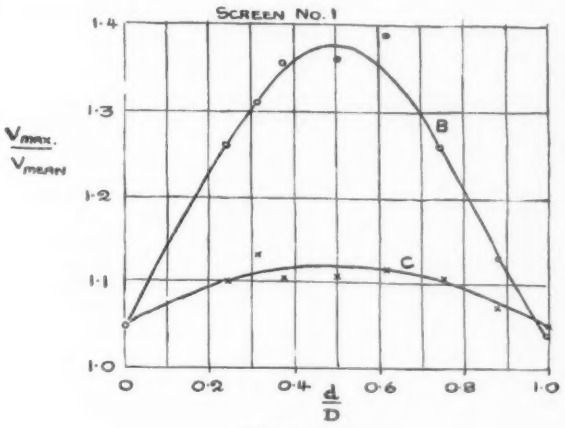

FIGURE 4(*a*).

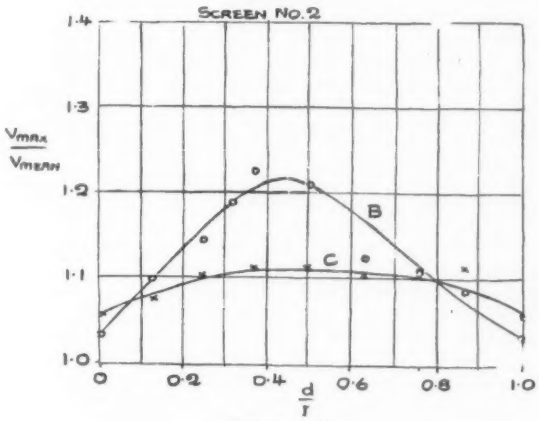

FIGURE 4(*b*).

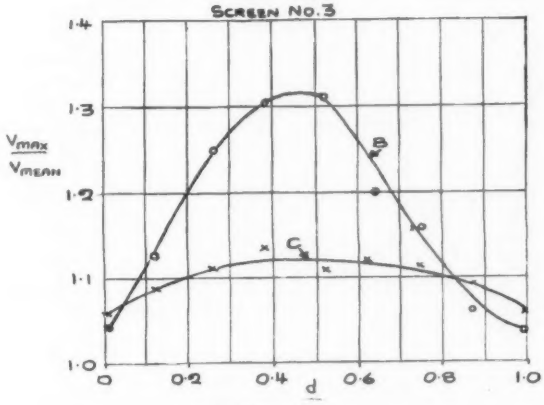

FIGURE 4(*c*).

free duct and a completely blocked duct respectively, while the limits of the open area ratio appropriate to Fig. 5 are the completely free duct and an orifice plate of the d/D ratio considered. The maximum value of V_{max}/V_{mean} obtained by these tests is of the order of 1·37 for an entry V_{max}/V_{mean} of 1·05. This may be compared with Livesey's figure of 1·42 obtained, however, with an inlet velocity profile of higher V_{max}/V_{mean} corresponding to fully developed turbulent flow in the duct.

The values of V_{max}/V_{mean} for the measuring position

C downstream show that there has been a rapid decay in the peakiness of the profile between sections B and C, and that there is no definite maximum value of V_{max}/V_{mean} as there is at section B.

Comparison with Livesey shows that the re-establishment of the flow downstream of the screen occurs much closer to the gauze than the measuring section B, and it may be that a higher value of V_{max}/V_{mean} can be measured at a point upstream of B. This may prove difficult in practice, however, owing to the rapid changes in velocity profile which occur immediately downstream of the screen, and it is felt that the simple concept of V_{max}/V_{mean} should be reserved for the profiles of a shape approximating to a smooth curve; it is not, therefore, the sole criterion for describing the flow close to the screen.

The pressure drop coefficients for the different screens cover very much the same range as those obtained by Livesey[3]. For the maximum V_{max}/V_{mean} ratio obtained, *i.e.* 1·37, the value is 2·5.

5. DISCUSSION

The foregoing tests have shown that it is possible to generate a highly peaked velocity profile in a duct by using wire gauze screens of different open area ratios, the screens being in a plane perpendicular to the axis of the duct and having central holes of different diameters. By a suitable choice of the dimensions of the screen, either in the open area ratios of the gauze or the central hole diameter, any peaked velocity profile may be generated as required with a low pressure loss.

The work may not be directly applicable to the flow in other duct systems, but it gives an indication of the results which may be expected and of the importance of the geometrical factors. It may be possible to generate more highly peaked velocity profiles by using an initially peaked velocity profile. This can be done by fitting two or more screens in series along the duct, and it is possible that there is an optimum spacing between the screens which would result in a maximum V_{max}/V_{mean}. Further tests on this aspect of the work are planned.

The decay of the velocity profile has already been mentioned and a preliminary investigation of this problem has been made. In connection with this work, it is clear that if an annular gauze screen is used to produce a peaked velocity profile, care must be taken not to position the equipment on which the effect of the V_{max}/V_{mean} ratio is to be measured too close to the generating screen, in order to obtain a highly peaked profile. On the other hand, it is also clear that the measurement of a peaked velocity profile at one section forward of the equipment is insufficient, and that a series of measurements must be made along the duct to enable a good extrapolation to be made along the duct to the actual inlet to the equipment. As

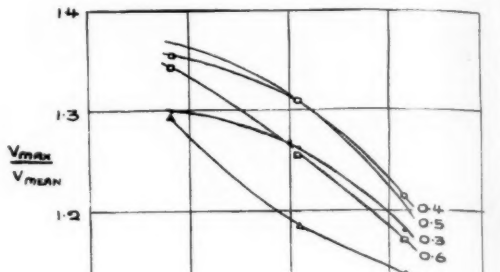

FIGURE 5.

mentioned in the introduction, artificially peaked profiles are usually produced in order to observe the effect on the performance of some component, such as a combustion chamber or diffuser. It would appear that the rate of change of V_{max}/V_{mean} may be as important as the actual value of V_{max}/V_{mean} itself, and further investigations into peaked velocity profiles may be directed towards this.

It is proposed to make a basic study of two-dimensional flow, to enable the results to have wider application. Particular attention will be paid to

(*a*) the decay of the velocity profile and the relation between profile itself and its rate of decay.

(*b*) the effect of an unsymmetrical approach profile to the screen.

(*c*) the effect of an unsymmetrical screen in the formation of an unsymmetrical profile, and the transverse changes in such a profile, as well as its longitudinal decay.

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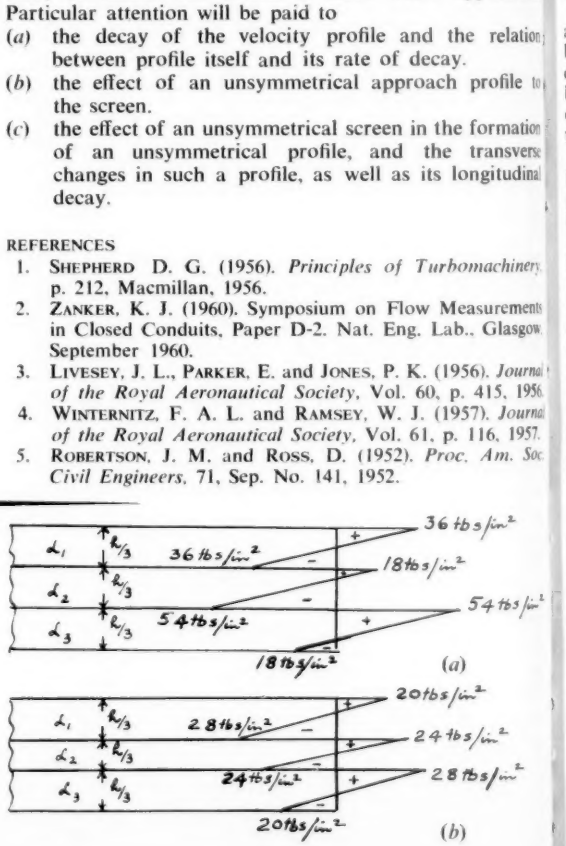
Thermal Bending of a Tri-Metal Strip

Erratum—p. 509, July Journal

In the Technical Note "Thermal Bending of a Tri-Metal Strip" by M. Vasudevan and W. Johnson, published in the July 1961 JOURNAL, Figure 3 (p. 509) was incorrect. A new diagram for this Figure was sent by the authors but, unfortunately, the blocks were mixed. The correct diagram of Figure 3 is as shown here and should be substituted.



Glasgow is the history of a Branch twice formed, in 1919 *and in* 1944.

The history of the original Glasgow Branch, from 1919 to 1926 was outlined in the JOURNAL for January, 1960. What was not mentioned was that the initial meeting of 1919 in Glasgow, was to honour the Officers and Crew of *H.M.A.* R34 for their Double Crossing of the Atlantic in the July of that year, and was attended by over 3,000, which probably constitutes the largest attendance of any Branch at any one meeting.

Undoubtedly the feat of R34 stirred great hopes in Scotland for the continuance of the then fairly large aeronautical industry. Airships for passenger purposes were actually being built, such as the R36, but the loss of the R38 in August 1921 brought complete cessation of airship building. This was followed by diminishing aviation activity in Scotland and the original Branch closed in 1926.

The 1939-45 war brought a considerable increase of aeronautical activity. Large numbers of Sunderland Flying Boats were built at the Blackburn Factory in Dumbarton, while extensive maintenance and repair facilities greatly extended the scope of Scottish Aviation Ltd. at Prestwick and Renfrew. Rolls-Royce opened up factories at Hillington and East Kilbride, Glasgow.

Many Society members found employment in Scotland and a desire to start a Branch found fulfilment in December 1944, when a new Glasgow Branch was set up centred chiefly around Prestwick. Mr. D. B. R. Briant, *Associate*, became Honorary Secretary. Numerical success was quickly attained, the membership rising to a peak of 435, to be short-lived with the end of the War in Europe.

In the following year, although there were 284 paid up Branch subscriptions, the record begins to complain of poor attendances at all functions, and by 1947 membership had fallen to 110.

This decline in interest continued and arose chiefly from the comparatively large distances between the centres of aeronautical activity in the area. At that time there was not a great deal of personal transport available while public transport was of such a character as to discourage movement except as an absolute must. There was no settled place of meeting, for, in an endeavour to suit everybody, meetings were held in Prestwick and various places in Glasgow, which meant a lot of movement for those wishing to attend. Despite these efforts no improvement resulted and it was seriously proposed to close down. It was decided to struggle on and hope for better times.

These came with the Fifties. Gradually it became the custom to hold the meetings in the Royal College of Science and Technology, Glasgow, which has been a good friend to the Branch. This is as central as possible for all concerned and the facilities for lectures are of the first order. Meetings were not always well attended at first, but with Professor W. J. Duncan, F.R.S., F.R.Ae.S., becoming President and Dr. A. W. Babister, M.A., A.F.R.Ae.S. of the Department of Aeronautics, Glasgow University, becoming Hon. Secretary, and the decision to invite lecturers from the main centres of the industry in the country, the better times materialised. But low troughs of interest were later experienced and once again the decision had to be made to carry on in hope and to work for a steady attendance. In 1955 the R38 Memorial Award was awarded to a Branch member for work in connection with airships, and in 1956 the R34 Memorial (which had been initiated by the Branch) was unveiled at East Fortune by the Duke of Hamilton. Mr. W. N. Alcock *Companion*, who, despite a very busy job, seemed always to have time in hand—became the Branch's representative at the Branch Conferences and so established a regular contact with the Conference. In 1957 he was elected Hon. Secretary, Dr. Babister became Chairman, with Professor Duncan remaining as President until his death in December 1960, which was such a blow to aeronautics as a whole.

Continuance of the policy of setting a high standard of lectures now began to show results, and a good standard of attendance and enthusiasm was experienced through the following years. In 1958 a Graduates' and Students' Section was formed on an autonomous basis. Dr. Babister became its Chairman in addition to being Branch Chairman, and Mr. G. H. K. Goold, B.Sc., *Graduate*, became Hon. Secretary of the Section. The meetings of the Section have been of a very high standard indeed and Glasgow has succeeded in an activity where others have failed.

Both the Branch and the Section have to face further difficulties owing to the recession of aeronautical employment in Scotland. They realise that under these conditions the fight to exist will be harder than ever. Glasgow has come through difficult times in the past, but heartened by substantial help from the Society (for which they ask to express their thanks on this Page) and counting on the personal enthusiasm of Members they intend to keep the Society's flag flying in Scotland.

It is alleged that the Society Secretary (also Chairman of the Branches Committee) has a proprietory interest in Glasgow and engineering, and it must be recorded that Mr. Alcock is also Hon. Secretary of the Federation of Engineering Societies in Glasgow—some 18 Societies with 9,000 members—in addition to the R.Ae.S. Branch.

If the future depends on interest, then that of Glasgow is surely secure.

BRANCH NEWS

Brough.—Two members of Blackburns—Mr. D. J. Whitehead and Mr. W. Bishop—have gained awards offered by the Royal Aeronautical Society.

Mr. Whitehead, Chief Test Pilot, won the Society's Branch prize for his Brough Branch lecture on "Deck Landing of Naval Aircraft." He received his award at the Society's Annual General Meeting on 4th May.

Mr. Bishop, an apprentice with Blackburns, has been awarded the N. E. Rowe Medal for the best lecture given before a Branch by a member under the age of 21 years. His paper was on "The Development of Tailless Aircraft and Flying Wings", for which he has already received the Brough Branch's prize of £5. He will receive the Medal at the Wilbur Wright Memorial Lecture in September.

Luton.—The Branch is coming up to its 21st Anniversary, and the President for 1961/62 is Mr. C. L. Cowdrey, F.R.Ae.S. (General Manager of the Napier Experimental Establishment at Luton), who was the original President at the formation of the Branch. 25 years is a long time in anybody's language: in aviation it is a very long time, and deserves both particular and general congratulation.

The Avro Avian

Readers may recall that the Branches Page made an appeal for help to save G-EBZM, which was in danger of being broken up. Two interested persons have responded, and I hope to publish good news in the future.—G. W-W.

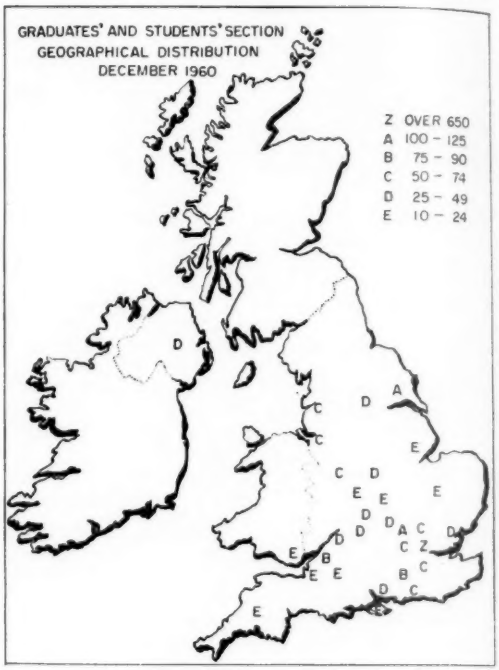
Graduates' and Students' Section

Geographical Distribution of Members

For a number of years the Section Committee has felt the need to do more to cater for those Graduate and Student members of the Society who live outside the London area and so are unable to take part in the various activities organised by the Committee in London. The intention has been to establish initially one or two sub-sections outside London in areas where there are sufficient members to give the necessary support to the venture.

With this in mind, earlier this year, the Committee analysed the registered addresses of the 2,020 members of the Section resident in the United Kingdom at the end of 1960. The final analysis is shown by the accompanying map. It was found that 658 members, or 33 per cent of the total, could get to the Society's headquarters in London for an evening lecture, without having to get time off. This was much to the relief of the Committee, some of whom were beginning to doubt the validity of their existence. Surrey and the South Coast, East Yorks. and Hull, and the Luton and Bedford areas all had over 100 members each. Bristol and Farnborough had 85 and 82 registered members respectively.

It would seem that the areas where most support could be given to a Graduates' and Students' Sub-Section are Bristol; Cranfield, Luton or Bedford; Brough and Farnborough. The present intention is to contact the Branch Secretaries in these and other areas in order to hear their views of the idea. In addition, the Committee would be very pleased to hear from others who are interested in the project.—W.G.W.



Report on Summer Dance

On Friday 23rd June, the Section held its annual summer dance at the Society headquarters in London. This was the first dance to be held in the Lecture Theatre and proved to be a pleasant and successful arrangement. The Section was honoured by a visit from the President and his wife, Sir Owen and Lady Jones.

The Graduates' and Students' Committee carried out a valuable piece of research for the Society during Friday afternoon. This involved removing the seats from the Lecture Theatre for the first time. The seats are built in banks of threes and fours and it was soon discovered that, although the seats are very comfortable and quite suitable for snoozing during less interesting lectures, they are not designed to aircraft standard as far as weight is concerned. There is just enough room to get the seats round the top of the stairs without removing paint or skin and the store room underneath the theatre holds about one-third of the seats, although more muscle power would probably allow double stacking. The remainder of the seats were arranged along the walls and in banks of four at right angles to the walls, thus providing ample seating for all present.

The music for dancing was provided by Hugh McCamley and it was generally agreed that the floor provided an excellent surface for waltzes, cha-chas, etc.

The bar was open in the Council Room throughout the evening, except for about 10 minutes while the food was laid out. Keg bitter, red wine, white wine and squashes were available, but the only drink finished was the squash. This was probably due to the rather high ambient temperature. Sandwiches, cakes and biscuits were also left over at the end of the evening.

Not only is there a considerable difference in the décor of the new Lecture Theatre from that of the library but it was also found that the method of maintaining a moderate temperature was entirely different. To reduce the temperature in the library one merely opened the windows; but in our new dance hall the air conditioning system maintained a constant temperature only so long as the doors were kept closed.

The thanks of all go to the wives and girl friends of the Section's Committee who so willingly gave a great deal of their time and energy to prepare the food for the buffet, thus reducing the cost of the dance considerably. Our thanks are also due to the members of the Society's staff who helped in numerous ways.—J.D.B.

Future Visits

A visit has been arranged to the Esso Research Laboratories at Abingdon, Berks. on Wednesday afternoon, 20th September 1961. A most interesting visit has been promised, so members are urged to book a place immediately. Apply to the Hon. Visits Secretary, N. R. Craddock, 5 Oxleay Road, Harrow, Middx. (Tel. Pinner 3633) **by the beginning of September at the latest.**

The Section has been invited to visit the National Gas Turbine Establishment at Pyestock, Farnborough, Hants. in the near future. If sufficient people are interested, the Hon. Visits Secretary will arrange a date on a Wednesday afternoon. Everyone interested please apply to N. R. Craddock.

Readers' Views

It has been the experience of Committee members that Students and Graduates of the Section often have some suggestion or criticism which helps the Committee in directing the activities of the Section. However, these opinions can usually only be obtained by sustained questioning of members while drinking pre-lecture coffee or during Section visits.

Furthermore, for this JOURNAL page to give as much information and aid as possible to the members of the Section, there must be a feed-back of information about the articles which have appeared. Even more important, what are the articles members would like to see?

Here is an opportunity for members, those too far from London to participate in most of the present activities of the Section, to take some part in shaping activities.

Write to the Section Editor *now*: P. Hampton, Staff Mess, Royal Aircraft Establishment, Farnborough, Hants.

THE LIBRARY

Reviews

FIELD COMPUTATIONS IN ENGINEERING AND PHYSICS. *A. Thom and C. J. Apelt. Van Nostrand, London,* 1961. 165 *pp. Diagrams.* 30*s.*

The authors define "field" problems as those which give rise to one or more partial differential equations in two-space variables. From this, it is clear that the reader should not expect a complete treatment of general three-dimensional time-dependent partial differential equations. However, he might reasonably expect to see a rather more comprehensive set of methods than the one propounded, together with comparisons of their relative merits.

The authors do give a clear and full account of the "squaring" method which has been applied successfully by Thom and others to many problems of fluid dynamics. There is sufficient introductory material on finite difference methods to make the book self-contained in this respect. The application of the method of squares to Laplace's and Poisson's equations is given in detail; both straight and curved boundaries are treated adequately. There is one chapter dealing with the biharmonic equation. A short chapter is devoted to questions of convergence and propagation of errors in Laplace and Poisson fields. A table is included which shows, among other things, the convergence rate as a function of the number of mesh points for four different finite difference formulae which are discussed in earlier chapters. The largest number of unit squares taken is 400 and, with the most favourable formulae, the convergence rate is only 0·892. This emphasises that the methods propounded here are of little value when thousands of mesh points have to be considered. Although the squaring method has been programmed for a Mercury computer, better methods are available for use with high speed computers, methods whose convergence rates are much less sensitive to the number of mesh points.

The student of fluid dynamics will be interested in the physical problems whose solution is discussed in detail. The flow of a perfect fluid along a constricted channel and round a right-angled corner is determined. The methods devised to deal with free stream lines are illustrated by considering a jet which impinges on a flat plate of finite width. There is a chapter on the flow of a compressible fluid and one on the solution of problems with axial symmetry.—G. N. LANCE.

THEORY OF DETONATION. *Ia. B. Zeldovich and A. S. Kompaneets. Academic, New York,* 1960. 284 *pp. Diagrams.* 10 *dollars.*

This book is an apparently anonymous translation from the Russian edition. No reference is given to either translator or technical editor. An editor's foreword to the translation would have proved valuable since it is obvious that the references given are limited, especially as to Western publications.

This point is referred to in the author's foreword where we learn that the book " . . . is the first systematic presentation of detonation theory based on papers completed at the Chemical Physics Institute (of the Academy of Sciences of the U.S.S.R.). This is probably the reason for the limited number of references to literature on the subject." This statement explains the scarcity of references only if we limit our consideration of the book to the research of this Institute, although it is clear both from the title and the form of presentation that its field is much wider. In an advanced textbook of this nature, we expect detailed references, both as sources and as a starting point for the more detailed investigation. This is especially so when, as in certain portions of the book, qualitative descriptions are given.

The reader who specialises in gas dynamics will welcome a presentation of fundamental detonation theory in the first few chapters which is not over-burdened with the details of the chemical reactions involved. To this end, the first chapter contains a description of one-dimensional gas dynamics in which the work of Sedov and Staniukovich is briefly described. The following chapter then describes detonation theory in gases without losses. The chemical kinetics involved are described briefly and it is here we would welcome references to a more exact treatment to substantiate some of the assertions made.

The effect of losses on the theory is then presented in a chapter for which the approach necessarily becomes more empirical and which finally describes the major deviation from one-dimensionality with "spin" detonation.

The more difficult problem of a condensed explosive is treated using the theory of Staniukovich, which utilises the simplifications of assuming the ratio of specific heats equal to three. Here, it is refreshing to encounter in what is very much an empirically developed theory, the statement "Having three parameters at our disposal, we can practically adjust any function to fit the experiments." At least, we feel that the authors are not over-zealous in convincing us of the applicability of the theory.

Finally, the motion of the detonation products in plane one-dimensional flow, in steady two-dimensional problems, are derived theoretically and a brief (and somewhat inadequate) reference is made to the spherically symmetric diverging detonation wave.

The book forms a very useful description of detonation theory, more especially as it is concerned with the Russian work. Some authoritative preface to the translation would have been valuable, however, to place the book in its Western context.—NEIL C. FREEMAN.

THEORY OF THERMAL STRESSES. *Bruno A. Boley and Jerome H. Weiner. John Wiley, New York (Chapman and Hall, London). 1960. 586 pp. Diagrams.* £6 4*s.* 0*d.*

Although the basic principles of Thermoelasticity were first presented by Duhamel as early as 1837, the theory thus arrived at has not been developed very much because of its limited application to practical problems. However, further interest in the theory of thermal stresses has been stimulated by the recent advance in nuclear reactor engineering, supersonic aircraft, missiles and spacecraft. In particular, the determination of thermal stresses caused by aerodynamic heating now plays an important part in the design of supersonic aircraft. Furthermore, the effects of temperature in the study of the inelastic behaviour of materials have also become of paramount importance in view of the high temperature and stress levels encountered in present-day aircraft structures.

While there is a great number of separate contributions on the subject in the technical literature, the book under review is the first attempt at presenting the fundamental principles of thermoelasticity and the methods of solution in a single source. It presents a unified treatment of the theory of thermal stresses, starting with the basic principles of thermodynamics, heat transfer theory, elasticity and creep, and then proceeding to the practical methods of solution used in the stress analysis.

In Chapter 1 the authors present the mathematical formulation of the behaviour of an elastic continuous medium under the combined action of heat and external loading. This is derived from the fundamental laws of mechanics and thermodynamics and it leads to the coupled thermoelastic boundary-value problem which presents considerable mathematical difficulty. Various simplifications are introduced in Chapter 2 to remove this difficulty and thus for most engineering applications uncoupled thermoelastic theory can be used without significant error.

In Chapter 3 the general three-dimensional stress problem is formulated explicitly in terms of either displacements or stresses as unknowns occurring in the coupled differential equations. A body force analogy (Duhamel's analogy) is also derived; it establishes correspondence between a thermal stress problem and a fictitious stress problem, involving body

and surface forces, at a uniform temperature. The plane strain and plane stress formulations are discussed in Chapter 4.

Chapters 5 to 7 give an account of heat transfer theory. This includes such topics as modes of heat transfer, Fourier conduction equation and the Uniqueness Theorem for heat flow. The discussion of the methods of solution, which follows, includes the method of imaging of sources and sinks, Green's functions, separation of variables, Laplace transformation, conformal mapping, numerical methods, electrical analogues and extension of solutions involving superposition of basic solutions, product solutions and Duhamel's theorem. The chapters on heat transfer provide an excellent background for the main topic of the book and help the interested reader with the methods of determining the temperature distribution which is the first prerequisite in any thermoelastic analysis.

Chapters 8 and 9 deal with some basic problems in thermoelasticity while Chapters 10 to 12 give practical solutions for thermal stresses in straight and curved beams, rings, trusses, frames and plates. Chapter 13 describes a number of problems on thermoelastic stability, which incidentally includes the familiar buckling of bimetallic strips used in the design of thermostats.

Chapters 14 to 16 are devoted solely to the thermal stress analysis in inelastic systems. These chapters form a useful summary of the existing knowledge and the latest developments in the important fields of plastic and viscoelastic stress analysis, including creep, which have established themselves firmly in structural design.

It is perhaps regrettable that no space has been devoted to the problem of thermal stresses in thin shells, which is of some importance in the design of missiles, but this criticism can in no way diminish the merit of this latest contribution to the theory of thermal stresses. The equation numbers in the text are preceded by the Section and Chapter numbers, and this leads, on occasions, to as many as five digit numbers! The reference to Section numbers seems to be superfluous, although it probably helped the authors in compiling their manuscript.

The book should be very useful as a reference work as well as a starting point for further investigations and to this purpose the annotated bibliography should be helpful in locating the additional information on a specific subject.—J. S. PRZEMIENIECKI.

HANDBOOK OF AVIATION METEOROLOGY. *Air Ministry Meteorological Office. H.M. Stationery Office*, 1960. 404 *pp. Illustrated.* 25*s.*

Sutcliffe's "Meteorology for Aviators" has stood the test of time remarkably well in view of the great advances in meteorological knowledge since it was published in 1940. The present volume is its successor and one is justified in expecting it, in 1960, to reveal these advances. We are largely disappointed because it does not bear the stamp of authority like its predecessor. It reads like a White Paper or committee report: it recounts second hand ideas and is not the effusion of its author's own thoughts: it plays safe—not that it ought to give an account of the latest theories as if they were gospel, but it could at least put them on a par with older theories which are no better. Age does not make a theory correct!

Thus, the account of the thunderstorm contains the findings of the "Thunderstorm Project" (1946) and its speculations as to the origin of the downdraughts is accepted; yet it has never been proved that a lapse rate exceeding the wet adiabatic in a cloud will produce downdraughts, and it is more probable that it will not. For an account of mountain and lee waves we are referred to a review of 1957, which has since been superseded in many respects. There is little about radar, which surely merits a consecutive section on its own instead of the fragments scattered through the book. It is asserted that the sudden change of lapse rate at the tropopause is not properly understood and implies that it is even unexpected, which is surely not the case.

Many of Sutcliffe's original diagrams are reproduced unmodified—for example, those showing turbulence over obstacles, which are archaic and could be improved considerably. The explanation of turbulence cloud at 900mb. does not bear examination even though it remains the official one!

In compiling the book the author has been very uncritical of his sources, and this is really the complaint; he has ignored many ideas of the last decade completely. The stamp of officialdom hangs heaviest on the collection of cloud photographs. The "stratocumulus cumulogenitus" is simply not what it is supposed to be: if anything it is cumulus stratocumulogenitus! Cumulus do not spread out at their bases as suggested in the caption. The "chaotic sky" is not chaotic to the discriminating eye. The "altocumulus castellanus" is a very bad example, if it can be called an example of this type at all. The "cirrus uncinus" is also poor, and the author has positively chained himself to the dreary definitions and captions of the International Cloud Atlas, and has made a selection of pictures greatly inferior to Sutcliffe's.

Having thus complained about this desk-ridden approach to a live, exciting, out-of-doors subject, it would be unfair not to recognise that into this volume has been condensed a vast amount of knowledge and experience. It has expanded rather than advanced Sutcliffe's original. In its scope it is undoubtedly unrivalled, and in many places where the treatment may be unsatisfactory no important facts are omitted. It is a book that must be recommended because of the vast amount of sound information about the behaviour of the atmosphere all over the globe. If readers must look further for understanding they will certainly be well briefed. One must hope that when its successor comes to be compiled the task will be divided among at least three authors and at least three volumes. The author is to be congratulated in that he has ranged over a field much wider than any normal author would dare to tackle.—R. S. SCORER.

PHYSICAL GAS DYNAMICS. *A. S. Predvoditelev. Pergamon, Oxford, 1961. 189 pp. Illustrated. 50s.*

This book, according to its dust jacket, contains the proceedings, translated from the Russian, of a Symposium devoted to work carried out in the Power Engineering Institute of the Academy of Sciences of the U.S.S.R. Since the volume itself contains no form of introduction or preface, the reader will not find the information within the book, nor, without diligent searching, will he be able to find out when much of the work was done. In some cases the work cannot be dated at all (this seems almost deliberate).

It seems likely that the volume is translated from "Physical Gas Dynamics and Combustion Problems"—collected works of the Combustion Physics Laboratory of the Power Engineering Institute, published in 1957.

The contents include papers on thermodynamic properties of air at temperatures up to 12,000°K (the results of this work have appeared elsewhere), transport properties of air at high temperature, effects of dissociation and ionization on flow behind normal shocks, nozzle flow, hydrodynamic phenomena accompanying electrical discharges in water and flame dynamics.

For its price the book is rather poorly produced by a photo-offset process. The photographic illustrations are virtually useless. At least one of the graphs has incorrect dimensions on the vertical axis.—B.E.B.

PRINCIPLES OF ASTRONAUTICS. *M. Vertregt. Elsevier, Amsterdam* (*van Nostrand, London*) 1961. 221 *pp. Diagrams.* 18*s.*

This short monograph has been translated from the Dutch. The English version on the whole reads well although here and there it strikes a rather odd note, *e.g.* "The solid propellant rocket is always ready to start . . ."

As a guide to astronautics this work, for its size, goes into considerable detail and a knowledge of mathematics to University Entrance standard is necessary, especially in the chapters on orbital theory. An introductory chapter on space travel philosophy is followed by five chapters devoted to rocketry and three on orbital mechanics; this from a total of eleven chapters.

The rocket sections embrace a history of rocket development, the first principles of rocket motion, the design of liquid and solid rockets and the use of rockets in booster systems, this last covering the theory of multiple stage launching, on which the author has published several papers.

The sections on orbital mechanics are more ambitious; one suspects that this may be the author's favourite topic. Once again first principles are invoked with the properties of conic sections and the derivation of Kepler's Laws proceeding to the study of orbits, the three body problem, and finally interplanetary paths and their optimisation. It is felt that perhaps this topic has been developed at the expense of other facets of space travel, such as the design and environmental problems associated with the capsule and its occupants. Finally there is a chapter that discusses the planets and their environment and another that considers future developments, in the propulsion field for example, and the horizons that will thus be opened up.

This book is reasonably priced and for its size covers useful ground in well presented detail. There are some 23 tables and 72 figures including solar system and artificial satellite data. With regard to the latter it is difficult to find two lists that agree completely on such items as perigee and apogee heights and payload; in particular payload should be defined more accurately. Incidentally on page 17 the payload is given incorrectly for Sputnik 2 although it is correctly quoted in the relevant table.

Because of its continental origin all measurements were originally expressed metrically. British equivalents are now given as alternatives, but there are many tables and figures where this has not been possible. The type of reader to whom this book will appeal may find this irritating.—C. R. TURNER.

Additions to the Library

Advances in Electron Tube Techniques. Proceedings of the Fifth National Conference, September 1960. *Edited* by David Slater. Pergamon, Oxford. 1961. 235 pp. Illustrated. £5. Presents, without discussion, the forty-four papers read at the Conference.

Aero-Space Medicine. Harry G. Armstrong. Bailliere, Tindall and Cox. London. 1961. 633 pp. Illustrated. £7 4s. 0d. *To be reviewed.*

4th AGARD Colloquium: Combustion and Propulsion. *Edited* by A. L. Jaumotte, A. H. Lefebvre and A. M. Rothrock. Pergamon, Oxford. 1961. 396 pp. Diagrams. £5 5s. Thirteen papers read at the Symposium in Milan in April 1960; grouped in seven sections: The Future of Air-Breathing Engines; Performance and Applications; Hypersonic Inlet Studies; Diffusion Flames and Detonation Waves; Nozzle Flow with Chemical Reactions; Research in Turbomachinery; High Temperature Material Problems. Verbatim discussions are included.

American Society for Testing Materials Proceedings Volume 60, 1960. Committee Reports, Technical Papers. American Society for Testing Materials, Philadelphia. 1961. 1240 pp. Illustrated. The Society does not, as a rule, receive these Proceedings. The A.S.T.M. sent this particular volume on request because it contains ten papers on fatigue (including "Design Applications for Improving Fatigue Resistance of Airplane Structures"), ten on high temperature and one on structural beryllium. There is an excellent subject index and author index.

ASTM Special Technical Publication No. 271: Adhesion and Adhesives. ASTM Special Technical Publication No. 279: Reinforced Plastics for Rockets and Aircraft. The American Society for Testing Materials, Philadelphia. 1961. 65 pp.; 134 pp. Illustrated. $2.25; $5.50. The first contains five papers, three of them presented during the Symposium on Adhesion and Adhesives at the Third Pacific Area National Meeting of ASTM in San Francisco, October 1959; all five are on metal-to-metal bonding. The second contains three papers on Reinforced Plastics and five on Thermal Ablation, from Symposia, with these titles, held also during the San Francisco meeting.

No. 284: Symposium on Acoustical Fatigue; No. 287: Symposium on Low-Temperature Properties of High-Strength Aircraft and Missile Materials; 'No. 289: Symposium on Shear and Torsion Testing. Published by the American Society for Testing Materials, Philadelphia. 1961. 65 pp.; 225 pp.; 120 pp. Illustrated. $2.50; $7; $4.75. Containing the papers, with discussions, presented at three Symposia, which were part of the Sixty-Third Annual Meeting of the American Society for Testing Materials, in Atlantic City in June 1960.

Boundary Layer and Flow Control. G. V. Lachmann. Pergamon, Oxford. 1961. 600 pp. Illustrated. 10 gns. *To be reviewed.*

Concepts from Tensor Analysis and Differential Geometry. Tracy Y. Thomas. Academic Press, London. 119 pp. 40s. Volume I of a new series of monographs, to present the theory of recent mathematical developments with applications in science and engineering.

Encyclopaedic Dictionary of Physics. J. Thewlis *et al.* (Editors). Pergamon, Oxford. 1961. 800 pp. Illustrated. £80 per set. *To be reviewed.*

Handbook of Thermophysical Properties of Solid Materials. Vol. I: Elements. Alexander Goldsmith, Thomas E. Waterman and Harry J. Hirschhorn. Pergamon, Oxford. 1961. 757 pp. Graphs. £33 for set of 5 volumes. A loose-leaf reference book of material compiled for the U.S.A.F. by the staff of the Armour Research Foundation. Based on literature published during the period 1940-47; materials covered include elements, alloys, ceramics, cermets, intermetallics, polymerics, and composite materials; properties include melting point, density, latent heats, specific heat, thermal conductivity, thermal diffusivity, emissivity, reflectivity, thermal expansion, vapour pressure, and electric resistivity.

High-Speed Testing, Vol. II. (A Symposium held at Boston, Mass., 27th January 1960.) Sponsored by Plas-Tech Corporation, Natick, Mass. Co-Chairmen, A. G. H. Dietz and F. R. Eirich. Interscience Publishers, New York 1961. 78 pp. Illustrated. $4.50. Eight papers by American authors, on the high-speed testing of metallic and non-metallic materials. One deals with textiles.

Long-Range Ballistic Missiles. Eric Burgess. Chapman & Hall, London. 1961. 255 pp. Illustrated. 35s. *To be reviewed.*

Methods of Celestial Mechanics. Dirk Brouwer and Gerald M. Clemence. Academic Press, London. 1961. 598 pp. £5 11s. 0d. A mathematical background for practical applications in the branch of astronomy devoted to the motions of celestial bodies. The only force dealt with is gravitation, except in one section which deals with atmospheric drag as it affects the motion of an artificial satellite.

Missile Configuration Design. S. S. Chin. McGraw-Hill, London. 279 pp. Diagrams. 85s. 6d. *To be reviewed.*

Modern Mathematics for Engineers. E. F. Beckenbach (Editor). McGraw-Hill, London. 1961. 456 pp. 74s. By sixteen different authors, this is the outcome of a lecture series presented by the University of California through University Extension, at five locations in the State during 1958-59. The object was to present "some exciting aspects of modern mathematics" to non-specialists with training in engineering or science, high-school and college teachers of mathematics, and others wanting to keep in touch with mathematical developments. The book is in three parts: mathematical methods; statistical and scheduling studies; physical phenomena.

Physical Chemistry in Aerodynamics and Space Flight: Proceedings of the Conference on Physical Chemistry in Aerodynamics and Space Flight. A. L. Myerson and

A. C. Harrison (Editors). Pergamon, London. 1961. 288 pp. Illustrated. £7. A Conference sponsored by the U.S. Air Force Office of Scientific Research and the Missile and Space Vehicle Department of the General Electric Company. The papers are fairly short and usually include discussions. The four sections are: Surface and Solid Phase Reactions; Gas Phase Reactions and Kinetics; Experimental Methods and Simulation; Thermodynamic and Transport Properties of Gases.

Physical Gas Dynamics. A. S. Predvoditelev. Pergamon, Oxford. 1961. 189 pp. Illustrated. 50s. *Reviewed in this issue.*

Praktische Mathematik für Ingenieure und Physiker. 3rd Edition. R. Zurmühl. Springer-Verlag, Berlin. 1961. 548 pp. Diagrams. DM 29.40. The third edition of a German textbook for engineers and physicists, previously published in 1953 and 1957. It covers the solution of equations, matrices, numerical and graphical integration, elementary statistics, Fourier series, Legendre polynomials and differential equations.

Progress in Solid Mechanics, Vol. II. I. N. Sneddon and R. Hill. North Holland Publishing Company, Amsterdam 1961. 331 pp. 70s. The second volume in a group of authoritative surveys of recent developments in the various branches of Solid Mechanics, both theoretical and experimental. The first volume was warmly recommended in a review in the *Aeronautical Quarterly* of November 1960 (p. 396). It is planned to range over the whole field in the early volumes. In this one the emphasis falls on recent outstanding advances in the two basic disciplines of elasticity and plasticity.

Second Annual Report, U.S. Federal Aviation Agency. U.S.G.P.O. (H.M.S.O.), Washington. 1960. 58 pp. 2s. 8d.

Some Special Problems Connected with Supersonic Transport. Bo Lundberg. F.F.A. (Sweden). 1961. 43 pp. Issued as F.F.A. Memo PE-11, this contains "comments on the 'Questionnaire' for the I.A.T.A. Symposium on Supersonic Air Transport in Montreal, April 1961." In this paper cosmic radiation and sonic booms are considered.

NATIONAL I A S - A R S JOINT MEETING
June 13th-16th 1961 Los Angeles

61-82-1776 An experimental evaluation of a two-dimensional ion engine with charge neutralization. R. J. Cybulski and D. L. Lockwood.
61-83-1777 Development of high efficiency cesium ion engines. M. P. Ernstene *et al.*
61-87-1781 Experimental effects of scaling on the performance of ion rockets employing electron-bombardment ion sources. P. D. Reader.
61-88-1782 Trends in air cargo. H. A. Carter and J. C. Jackovich.
61-89-1783 An examination of the economic and technical factors underlying the future U.S. domestic market for air transportation service. G. A. Busch and K. M. Mayer.
61-91-1785 Air trailer transportation. W. M. Ligon.
61-92-1786 Economics of air eligibility. G. L. Harris and L. G. Regan.
61-93-1787 Prediction, improvement and measurement of reliability of one-shot devices. L. W. Ball.
61-95-1789 Maximizing reliability for one-shot space missions. S. C. Morrison.
61-96-1790 One-shot mission success for minimum cost. W. E. Cox and W. W. Harter.
61-97-1791 Regenerative and radiation cooling of electrothermal thrust generators. J. R. Jack.
61-99-1793 Performance characteristics of the 3,000 cycle electrothermal rocket. R. M. Spongberg.
61-100-1794 Recent experimental plasmajet thrustor results. P. S. Masser.
61-103-1797 Theoretical and experimental description of the oscillating electron ion engine. J. W. Davis *et al.*
61-104-1798 Short haul air transportation. W. P. Kennedy.
61-105-1799 Aerodynamic characteristics of subsonic V/STOL transport airplanes. M. W. Kelly and C. A. Holzhauser.
61-106-1800 Application of laminar flow control to transport aircraft. W. E. Gasich.
61-107-1801 Hypersonic airplane performance. A. A. du Pont.
61-108-1802 Neutron-gamma ray instrumentation for lunar surface composition analysis. C. D. Schrader *et al.*
61-110-1804 Application of gas chromatography to the analysis of organics, water, and adsorbed gases in the lunar crust. V. I. Oyama.
61-112-1806 Electronic ultra-violet imaging techniques. S. Gray.
61-114-1808 Supercircular entry and recovery with maneuverable manned vehicles. R. H. Smith and J. A. Menard.
61-115-1809 Prediction of optimum approach and landing techniques for manned re-entry gliders. L. M. Gaines and T. E. Surber.
61-116-1810 A second order theory of entry mechanics into a planetary atmosphere. W. H. T. Loh.
61-118-1812 Instrument flight simulator study of the VTOL controllability—control power relationship. J. Patierno and J. A. Isca.
61-119-1813 Design characteristics of a lifting fan V/STOL logistic transport. E. Kazan and W. Bergen.
61-120-1814 The influence of two-dimensional stream shear on airfoil maximum lift. R. J. Vidal.
61-121-1815 The design of a supersonic V/STOL fighter-bomber. J. O. O'Malley.
61-123-1817 The age concept of management for the ground environment area. S. A. Tremaine.
61-124-1818 Universal program-comparator checkout equipment and logistical considerations. E. Metsker and M. Beeson.
61-125-1819 The role of operational analyses in planning an effective missile ground system. S. I. Firstman.
61-126-1820 Compiling of automatic test equipment programs. E. B. Quinn and C. F. Chipps.
61-128-1822 The influences of solid propellant burning rate exponents on the performance of engines with large back pressure variations. R. C. Brumfield.
61-129-1823 A study of solid-fuel ramjet applications. P. L. Munter.
61-130-1824 Main tank injection missile pressurization systems. H. B. Allison and C. D. Bailey.
61-131-1825 Effect of rapid pressure decay on solid propellant combustion. C. C. Ciepluch.
61-132-1826 Steady-state combustion measurements in a LOX/RP-1 rocket chamber and related spray burning analysis. S. Lambiris and L. P. Combe.
61-133-1827 Computers as an aid to the speed and accuracy of management decisions in the space age. W. T. Mason.
61-134-1828 A description of the weapons assignment research model (WARM). R. A. Totschek.
61-135-1829 A general purpose simulator for airborne computers. E. V. Carter.
61-136-1830 A new scientific management technique for the Air Force. L. A. Ohlinger.
61-139-1833 Observation satellite orbits. L. G. Vargo *et al.*
61-140-1834 Differential correction of earth satellite orbits including the drag perturbation. C. T. Van Sant and G. Westron.
61-141-1835 Orbit determination of a non-transmitting satellite using Doppler tracking data. P. B. Richards.
61-143-1837 Design alternatives for in-flight maintenance. R. G. Demaree.
61-144-1838 Human engineering principles of design for in-space maintenance. L. D. Pigg.
61-145-1839 The use of tools in space—an empirical approach. J. T. Celentano and H. S. Alexander.
61-146-1840 Predicting human reliability-implications for operations and maintenance in space. R. A. Kaufman *et al.*
61-147-1841 Some experiments of human maintenance behavior. M. A. Grodsky.
61-148-1842 On the ultimate tensile strength and elongation of ductile materials. F. R. Shanley.
61-149-1843 A unified engineering theory of high stress level fatigue. S. R. Valluri.
61-150-1844 Structural aspects of low drag suction airfoils. J. Wieder and W. Pfenniger.
61-151-1845 Leading edge design with brittle materials. F. M. Anthony and A. L. Mistretta.
61-153-1847 A generalized three-dimensional trajectory analysis of planetary escape by solar sail. W. R. Fimple.
61-154-1848 Correction for the effects of finite thrusting time in orbit changing maneuvers. W. O. Darby.
61-155-1849 An attenuated intercept satellite rendezvous system. M. Shapiro.
61-159-1853 Sealed cabin experimentation. B. E. Welch *et al.*
61-161-1855 Crew physical support and restraint in advanced manned flight systems. H. Smedal *et al.*
61-162-1856 The mini-recorder system. J. B. Renfro.
61-163-1857 Radiation shielding and manned satellite design considerations. L. J. Barbieri and S. Lampert.
61-164-1858 Reliability in space. E. D. Karmiol and J. S. Youtcheff.
61-176-1870 Instrumentation for the gamma ray astronomy satellite, S-15. I. The telescope. II. The telemetry. W. L. Kraushaar, G. W. Clark, and O. B. King.
61-177-1871 A mass spectrometer for upper-air measurements. E. J. Schaefer and M. H. Nicholls.
61-179-1873 A solar probe. D. H. Dickstein.
61-180-1874 A description of a Mars spacecraft with a landing capsule. C. R. Gates.
61-183-1877 Directive non-orientated reflectors as passive satellites in long distance communications. Y. E. Stahler and A. L. Johnson.
61-184-1878 Special applications of 24-hour communication satellites. J. H. W. Unger.
61-185-1879 Television broadcasting from satellites. G. M. Ives.
61-188-1882 Electromagnetic power systems for space applications. N. W. Bucci and R. W. Briggs.
61-189-1883 Experimental results of a cryogenic heat exchanger. W. F. Chew.
61-190-1884 The magnetic particle clutch—a versatile control element for rocket systems. R. Grau and B. A. Chubb.
61-193-1887 Polaris digital and analog hardware simulation. C. H. Kaufmann.
61-194-1888 X-15 flight simulation program. N. R. Cooper.
61-195-1889 Piloted simulation studies of re-entry guidance and control at parabolic velocities. R. C. Wingrove and R. E. Coate.
61-196-1890 The simulator as a human factors research tool for manned space flight. G. B. Simon.
61-200-1894 Can satellite communication systems share frequencies with surface radio services? S. G. Lutz.
61-204-1898 A simple system for sun orientation of a spinning satellite. T. G. Windeknecht.
61-205-1899 Attitude control rocket requirements for space vehicles. C. N. Tripp and W. P. Boardman.
61-207-1901 Attitude control system for a spinning body. L. E. Freed.
61-208-1902 Flow field analysis for lifting re-entry configurations by the method of characteristics. L. R. Fowell.
61-209-1903 Calculation of hypersonic flow past spheres and ellipsoids. M. Holt and G. H. Hoffman.
61-210-1904 Flow past slender blunt bodies—a review and extension. R. Capiaux and L. Karchmar.
61-212-1906 The applicability of the hypersonic similarity laws to a complete missile configuration. J. W. Hindes and N. R. Augustine.
61-213-1907 Heat transfer to slender cones in hypersonic air flow including yaw and nose-bluntness effects. C. E. Wittliff and M. R. Wilson

S.A.E. PREPRINTS
S.A.E. Summer Meeting 1961

359B Fluid couplings. J. W. Qualman and E. L. Egbert.
365A Special aspects of structural design for safety of light aircraft. K. H. Bergey.
365C The Beech turboprop travel air test bed—a progress report. M. J. Gordon.
370A VTOL wind tunnel testing techniques and facilities. R. C. McWherter.
370B Some experiences with ground effect devices. C. A. Amann and J. W. Scheel.
370C Gust simulation as applied to VTOL control problems. W. J. Klinar and S. J. Craig.

373A Scuffing of porous chrome-plated R-4360 cylinders. G. C. Lawrason *et al.*
373B Allison unit exchange plan for Electra engines and propellers. R. W. Sherk.
373C TWA's engine overhaul system. L. Spraill and T. B. King.
380C The influence of compression ratio and dissociation on ideal Otto cycle engine thermal efficiency. M. H. Edson.
380D Lean fuel/air mixtures for high-compression spark-ignited engines. J. A. Bolt and D. H. Holkeboer.
380E Nitrogen oxides and engine combustion. D. B. Wimmer and L. A. McReynolds.
380F A new I.F.P. process for engine combustion: a variable air-fuel ratio spark ignition engine J. Baudry.
382B Gas turbine shaft dynamics. G. Hamburg and J. Parkinson.
383A Field experience with GMT-305 gas turbine in military applications. R. W. Guernsey.

Reports

AERODYNAMICS

BOUNDARY LAYER *see also* STABILITY AND CONTROL
THERMO-AERODYNAMICS
TESTING AND INSTRUMENTS

Laminar boundary layer behind a strong shock moving into air. H. Mirels. N.A.S.A. T.N. D-291. Feb. 1961.
Numerical results have been obtained for shock Mach numbers up to 14, using real gas values for density and viscosity and assuming Prandtl and Lewis numbers of 0·72 and 1, respectively.—(1.1.1.4×1.2.3.2).

The flow field over blunted flat plates and its effect on turbulent boundary-layer growth and heat transfer at a Mach number of 4·7. *T. Tendeland* et al. *N.A.S.A. T.N. D-689. Feb.* 1961. —(1.1.3.4×1.9.1).

A calculation method for three-dimensional turbulent boundary layers. J. C. Cooke. R. & M. 3199. 1961.—(1.1.3.1).

Measurements of the surface pressure fluctuations in a turbulent boundary layer in air at supersonic speeds. D. J. M. Williams. A.A.S.U. Report 162. *Dec.* 1960.
Experiments are described to measure the root mean square turbulent pressure fluctuations on the plane wall of a supersonic nozzle.—(1.1.3.4×1.5.1×1.6.3).

Approximate solutions of the hypersonic laminar boundary layer equations with heat transfer and arbitrary pressure gradient and their applications. H. Naruse. Aero. Res. Inst. Report 361. *Feb.* 1961.—(1.1.1.4×1.9.1).

Stability of the compressible boundary layer along a curved wall under Görtler-type disturbances. Y. Aihara. Aero. Res. Inst. Tokyo. Report 362. *Feb.* 1961.—(1.1.1.2×1.1.1.4).

COMPRESSIBLE FLOW *see also* BOUNDARY LAYER
LOADS
THERMO-AERODYNAMICS

A method for designing body shape to produce prescribed pressure distributions on wing-body combinations at supersonic speeds. J. G. Jones. C.P. 540. 1961.—(1.2.3.1×1.10.1.2).

The flow of a gas at hypersonic speed around conical bodies. A. L. Goner. R.A.E. Lib. Trans 935. *February* 1961.
Solutions are obtained for a plane delta wing and for elliptic cones. The pressure over the body, the shock wave and the effect of thickness variation are covered.—(1.2.3.1×1.10.1.2).

A method of solution with tabulated results for the attached oblique shock-wave system for surfaces at various angles of attack, sweep, and dihedral in an equilibrium real gas including the atmosphere. R. L. Trimpi and R. A. Jones. N.A.S.A. T.R. R-63. 1960.
Results are tabulated for the following ranges: angle of attack, 0° to 65°, angle of sweep, 0° to 75°, angle of dihedral, 0° to 30°, Mach number, 3 to 30; and "effective specific-heat ratio" parameter, 1·10 to 1·67. Both the method and tabulated solutions are easily adaptable to flight in any gas or in the atmosphere of any planet. An illustrative example is presented based on the 1956 A.R.D.C. model atmosphere.—(1.2.3.2).

A free-flight investigation into the effect of body shaping on the zero-lift drag of a wing-body combination at transonic and supersonic speeds. J. A. Hamilton et al. *R. & M.* 3193. 1961.
Ten configurations were flown, three had unwaisted bodies, five were designed by area-rule methods and two were designed to achieve a specified pressure distribution in the wing-body junction. All had identical values of total volume (wing-body), body length and body base area; the wing design was also common (45° sweep, aspect ratio 2·4, thickness/chord ratio 0·074 no taper). The Mach number range was from 0·8 to 1·5.—(1.2.2.1×1.2.3.1).

Atomic recombination in nozzles: methods of analysis for flows with complicated chemistry. K. N. C. Bray and J. P. Appleton. A.A.S.U. Report 166. *April* 1961.
This reviews the feasibility of performing accurate calculations on the non-equilibrium flow of a complicated gas mixture through a nozzle, particularly the propulsive nozzle of the hypersonic ramjet. Equations are given for the steady, quasi-one-dimensional, adiabatic flow of a general reacting gas mixture, and methods of solution are discussed. The formulation of the system of chemical reactions is discussed.—(1.2.3.1×1.5.1×27.4×32.1).

CONTROLS *see* LOADS
STABILITY AND CONTROL
WINGS AND AEROFOILS

FLUID DYNAMICS *see also* THERMO-AERODYNAMICS
EXTRA-ATMOSPHERIC TECHNOLOGY

A method of calculating the velocity distribution on annular aerofoils in incompressible flow. J. A. Bagley et al. *R. & M.* 3146. 1961.
An annular wing at zero incidence is treated first, and then the wing at incidence. Examples of the calculated velocity distributions on various aerofoils are given.—(1.4.1×1.10.1).

INTERNAL FLOW *see also* BOUNDARY LAYER
COMPRESSIBLE FLOW
TESTING AND INSTRUMENTS

The performance of a cascade fitted with blown flaps. R. A. Kruger et al. *C.P.* 526. 1961.—(1.5.4.3×1.5.4.1).

The use of woollen felt screens as air cleaners for supersonic wind tunnels. J. F. W. Crane. C.P. 538. 1961.
Measured values of the pressure drop coefficient are given over a range of values of Reynolds number that should be sufficient for most applications.—(1.5.1.3×1.12.1.3).

One dimensional irreversible gas flow in nozzles. R. A. A. Bryant. University of New South Wales Bulletin 3. *Dec.* 1960.
The objective is to give a concise summary of one-dimensional nozzle flow theories and to point out inadequacies.—(1.5.1).

Die strömung in vielstufigen Axialturbinen mit gerader Beschaufelung. K. Bammert. D.F.L. Bericht 135. 1961.—*(In German.)*—(1.5.3.1).

LOADS *see also* BOUNDARY LAYER
AEROELASTICITY
FLIGHT TESTING
STRUCTURES—LOADS

An investigation of wing and aileron loads due to deflected inboard and outboard ailerons on a 4*-per cent-thick* 30° *swept-back wing at transonic speeds. C. F. Whitcomb* et al. *N.A.S.A. T.N. D-620. Jan.* 1961.
The model was a sting-mounted wing-body combination; pressure measurements over one wing panel and one inboard and two outboard ailerons were obtained for angles of attack from 0° to 20° at deflections up to ±15°.—(1.6.1×1.3.1.1×33.1.1).

Pressure measurements on sharp and blunt 5°- *and* 15°*-half-angle cones at Mach number* 3·86 *and angles of attack to* 100°. *J. L. Am'ck. N.A.S.A. T.N. D-753. Feb.* 1961.
By combining empirical results for cylinders normal to the flow with Newtonian concepts, a method of calculating pressures on cones at high angles of attack is developed.—(1.6.1×25.2×1.2.3.2).

PERFORMANCE *see* FLIGHT TESTING

STABILITY AND CONTROL *see also* WINGS AND AEROFOILS
AIRCRAFT OPERATION
FLIGHT TESTING
MATHEMATICS

Status of spin research for recent airplane designs. A. I. Neihouse et al. *N.A.S.A. T.R. R-57.* 1960.
The study is presented in terms of the following interpretation

of results of spin-model research, analytical spin studies, techniques involved in measurements of parameters in the spin, effectiveness of controls during spins and recoveries, influence of long noses, strakes, and canards on spin and recovery characteristics, and correlation of spin and recovery characteristics for recent aeroplane and model designs.—(1.8.3.1).

Determination of lateral stability characteristics from free-flight model tests, with experimental results on the effects of wing vertical position and dihedral at transonic speeds. C. L. Gillis et al. *N.A.S.A. T.R. R-65.* 1960.—(1.8.1.2×13.2×1.12.2).

Experimental investigation of a hypersonic glider configuration at a Mach number of 6 and at full-scale Reynolds numbers. A. Seiff and M. E. Wilkins. N.A.S.A. T.N. D-341. Jan. 1961.
A hypersonic aeroplane configuration of three triangular wing panels and a body of equal length is investigated by gun-launched models. Characteristics investigated included minimum drag with rough and smooth model surfaces, lift-curve slope, and static and dynamic stability.—(1.8.0.2×3.3×25.2).

Lateral stability and control characteristics of a four-propeller deflected-slipstream V.T.O.L. model including the effects of ground proximity. R. E. Kuhn and K. J. Grunwald. N.A.S.A. T.N. D-444. January 1961.
The investigation covered the transition speed range from hovering to normal flaps-retracted flight. The tests covered the effects of flap deflection, thrust coefficient, aileron deflection, rudder deflection, and ground proximity.—(1.8.1.2×1.3).

Investigation of longitudinal and lateral stability characteristics of a six-propeller deflected-slipstream V.T.O.L. model with boundary-layer control including effects of ground proximity. K. J. Grunwald. N.A.S.A. T.N. D-445. January 1961.
The high-wing configuration had a 50 per cent chord sliding flap and a 30 per cent chord Fowler flap to deflect the propeller slipstream for vertical take-off and landing. The tests covered the transition speed range from hovering to normal flight with flaps retracted. Variables were flap deflection, thrust coefficient, stabiliser incidence, ground proximity, and blowing boundary layer control effectiveness.—(1.8.0.2×1.1.6.1×1.3).

Effects of various arrangements of slotted and round jet exits on the lift and pitching-moment characteristics of a rectangular-base model at zero forward speed. R. D. Vogler. N.A.S.A. T.N. D-660. February 1961.—(1.8.2.2×27.1.1.1).

Rocket-model investigation of lateral stability characteristics and power effects of a jet-engine airplane configuration with tail boom at Mach numbers from 1·15 to 1·37. T. B. Curry and N. L. Crabill. N.A.S.A. T.N. D-638. 1961.—(1.8.1.2×27.3).

Effect of flare on the dynamic and static moment characteristics of a hemisphere-cylinder oscillating in pitch at Mach numbers from 0·3 to 2·0. J. G. LaBerge. N.R.C. Report L.R.-295. January 1961.—(1.8.0.2).

Calculation of non-linear aerodynamic stability derivatives of aeroplanes. K. Gersten. D.F.L. Bericht 143. 1961.—*(In English.)*
Existing methods for calculating the non-linear effects are studied, and the different methods with their different vortex conceptions compared. A method for calculating the lift distribution of wings with sharp leading edges in incompressible flow is presented.—(1.8.2.2).

THERMO-AERODYNAMICS *see also* BOUNDARY LAYER

Exploratory study of the reduction in friction drag due to streamwise injection of helium. B. L. Swenson. N.A.S.A. T.N. D-342. January 1961.—(1.9.1×1.1.3.4×1.1.1.4×1.4).

Heat transfer to surfaces of finite catalytic activity in frozen dissociated hypersonic flow. P. M. Chung and A. D. Anderson. N.A.S.A. T.N. D-350. January 1961.—(1.9.1).

Heat-transfer measurements at a Mach number of 4·95 on two 60° swept delta wings with blunt leading edges and dihedral angles of 0° and 45°. P. C. Stainback. N.A.S.A. T.N. D-549. Jan. 1961.
The laminar-flow heat transfer distribution (ratio of local to stagnation-line heating rate) around the wing normal to the leading edge and the stagnation line heat transfer level were compared with two-dimensional blunt-body theory. Heat-transfer levels to the two wings were compared at equal angles of attack and equal lifts. The tests were made at a stagnation temperature of 400°F, and the test-section unit Reynolds number was varied from $1{\cdot}95\times10^6$ to $12{\cdot}24\times10^6$ per ft. The angle of attack was varied from 0° to 20° for the two configurations.—(1.9.1).

Heat-transfer measurements on the apexes of two 60° swept-back delta wings (panel semiapex angle of 30°) having 0° and 45° dihedral at a Mach number of 4·95. C. R. Gunn. N.A.S.A. T.N. D-550. Jan. 1961.—(1.9.1×1.10.2.2).

Effect of shock impingement on the distribution of heat-transfer coefficients on a right circular cylinder at Mach numbers of 2·65, 3·51 and 4·44. R. A. Newlander. N.A.S.A. T.N. D-642. Jan. 1961.—(1.9.1×1.2.3.2).

Application of similar solutions to calculation of laminar heat transfer on bodies with yaw and large pressure gradient in high-speed flow. I. E. Beckwith and N. B. Cohen. N.A.S.A. T.N. D-625. Jan. 1961.—(1.9.1×1.1.1.4).

Heat transfer and laminar-boundary-layer separation in steady compressible flow past a wall with non-uniform temperature. N. Curle. R. & M. 3179. 1961.—(1.9.1×1.1.4.4).

WINGS AND AEROFOILS *see also* COMPRESSIBLE FLOW
FLUID DYNAMICS
THERMO-AERODYNAMICS
AEROELASTICITY

Exploratory tests on a thin delta wing in the flow field of a rectangular foreplane at Mach number 1·8. M. S. Iggleston and L. J. Beecham. C.P. 530. 1961.
Normal force and pitching moment were measured at Mach number 1·8 on a delta wing in the field of a lifting foreplane.—(1.10.2.2×1.8.2.2).

Ground effect on a 55° swept M-wing of aspect ratio 5·0. L. A. Wyatt. C.P. 541. 1961.
The results of low-speed tunnel tests to determine the effects of ground proximity on the lift, drag and pitching moment characteristics of a flat-plate *M*-wing. A few results are included to indicate how the presence of the ground modifies the effectiveness of an unswept tailplane mounted at varying heights relative to the wing chord plane.—(1.10.2.2×1.8.2.2).

Theoretical pressure distributions on wings of finite span at zero incidence for Mach numbers near 1. A. Y. Alksne and J. R. Spreiter. N.A.S.A. T.R. R-88. 1961.—(1.10.1.2).

Ground influence on a model airfoil with a jet-augmented flap as determined by two techniques. T. R. Turner. N.A.S.A. T.N. D-658. Feb. 1961.—(1.10.2.1×1.3.4).

HELICOPTER AERODYNAMICS

Wind tunnel experiments on a model of a tandem rotor helicopter. A. S. Halliday and D. K. Cox. C.P. 517. 1961.—(1.11.3).

Response of a helicopter rotor to an increase in collective pitch for the case of vertical flight. J. Rebont et al. *N.A.S.A. T.T. F-55. January* 1961.—(1.11.3).

TESTING AND INSTRUMENTS *see also* INTERNAL FLOW
STABILITY AND CONTROL
SCIENCE—GENERAL

On the measurement of local surface friction on a flat plate by means of Preston tubes. 1961. *Staff Aero. Div. N.P.L. R. & M.* 3185. 1961.
Measurements of local surface friction were made by Preston's method using a round Pitot attached to the surface ("Preston tube") and were compared with the values given by differentiation of overall measurements of surface friction by the wake-traverse method and with determinations by means of surface Pitots of the type used by Stanton.—(1.12.5×1.1.3.1).

Comparison of the aerodynamic characteristics of AGARD Model A from tests in 12-in. and 40-in. supersonic wind tunnels. C. J. Schueler. A.E.D.C.-T.N.-61-8. February 1961.
The tests covered Mach number range from 1·5 to 5, Reynolds number range from 10^6 to $1{\cdot}3\times10^7$ based on the body length and an angle-of-attack range from −4 to 6°. Measurements were obtained to evaluate the AGARD model specification for sting length and diameter. Lift, drag, and pitching-moment results are compared, emphasising the importance of transition measurements.—(1.12.1.3).

Air liquefaction effects on the aerodynamic characteristics of the AGARD calibration Model B at Mach numbers 5 and 8. C. R. Fitch et al. *A.E.D.C.-T.N.*-61-24. *February* 1961.
Force and pressure distribution data were obtained at Reynolds numbers ranging from $6{\cdot}5\times10^6$ to 22×10^6 based on model length. The test was conducted at temperatures from above to well below the theoretical saturation values.—(1.12.1.3).

Instrumentation for and preliminary measurements of space-time correlations and convection velocities of the pressure field of a turbulent boundary layer. M. K. Bull. U.S.A.A. Report 149. *August* 1960.
Details of the transducers and other equipment for measurement of the space-time correlations of the pressure field of the turbulent boundary layer on the wall of the 6in. × 2½ in. wind tunnel at Southampton University.—(1.12.6.1×1.1.3.2×1.5.1.4).

AEROELASTICITY

The measurement of sub-critical damping on the R.A.E. flutter simulator. J. Appleton and W. D. Hicks. C.P. 529. 1961.
Electronic equipment installed in the R.A.E. six-degree-of-freedom flutter simulator enables the damping of a flutter problem at air speeds below the critical flutter speed to be obtained more readily than with previous methods. The theory of the method, details of the installation and results obtained are given.—(2.18.1×33.1.2).

Vectorial analysis of flight flutter test results. E. G. Broadbent and E. V. Hartley. R. & M. 3125. 1961.
A binary flexure-torsion analysis has been made to check theoretically a method of the prediction of flutter which depends on plotting vectorially the amplitudes of response relative to the exciting force and extracting the relevant damping rate. Results are given in graphs of the vector plots and of the estimated damping rate against forward speed. The estimated damping rates are compared values.—(2×13×33.1).

Aerodynamic derivative measurements on a rectangular wing of aspect ratio 3·3. P. R. Guyett and J. K. Curran. R. & M. 5171. 1961.
A complete set of oscillatory aerodynamic stiffness and damping derivatives has been determined for a rectangular wing for rigid-wing modes of normal translation, pitch, and roll, in the range of frequency parameter 0·4 to 1·3 at low subsonic wind speeds.—(2×1.10.2.2×1.6.3).

AIRCRAFT

See AERODYNAMICS—STABILITY AND CONTROL
AIRCRAFT OPERATION
FATIGUE

AIRCRAFT OPERATION

See also STRUCTURES—LOADS
POWER PLANTS

Damage incurred on a tilt-wing multipropeller V.T.O.L./S.T.O.L. aircraft operating over a level, gravel-covered surface. R. J. Pegg. N.A.S.A. T.N. D-535. *Dec.* 1960
Damage was incurred by a tilt-wing type V.T.O.L. aircraft which was inadvertently operated from a level macadam runway covered with loose and embedded gravel.—(5×3.12×13.3).

Random excitation of a tailplane section by jet noise. B. L. Clarkson and R. D. Ford. A.A.S.U. Report 171. *March* 1961.
A tailplane section was subjected to jet-noise and discrete frequency noise and the levels and correlation patterns of the jet noise field measured. The strains in the tailplane skin panels under both random and harmonic excitation were measured and compared. The mode shapes of the skin vibration were determined with the aid of strain-gauges, observed visually and measured photographically.—(5.6×32.2.3).

A comparison of three visual glidepath systems. J. R. Baxter et al. *A.R.L. Note H.E.* 8. *Oct.* 1960.
An experiment was made to compare three systems of visual glidepath guidance: the Visual Glide Path Indicator (V.G.P.I.), the Precision Visual Glidepath (P.V.G.), and the newer Tee Visual Glidepath (T.V.G.). Eleven airline captains and four military test pilots acted as test subjects, making three approaches by day and two by night on each system. Their performance was recorded by theodolite and their preferences and comments on the three systems elicited by means of interviews after both day and night flights.—(5.3×5.5).

Sea trials of the tumbling aerofoil crash position indicator. H. T. Stevinson et al. *N.R.C. Report L.R*-292. *Dec.* 1960.
To evaluate the seaworthiness and range obtainable with the N.R.C. tumbling aerofoil type of crash position indicator beacon in both calm and rough sea conditions, tests were conducted 30 miles off the coast of Halifax, Nova Scotia. An aircraft search height of 9000 ft. was used to obtain both polar patterns and extreme range information.—(5.3).

Calculation of symmetric response of a jet fighter to atmospheric turbulence and instrument landing system disturbances during approach for landing. A. Hyden and M. Sundström. K.T.H. Aero T.N. 50. 1960.—(5×1.8.0.1).

EXTRA-ATMOSPHERIC TECHNOLOGY

See also POWER PLANTS

Radio-frequency mass spectrometer for the investigation of the ionic composition of the upper atmosphere. V. G. Istomin. N.A.S.A. T.T. F-53. *Jan.* 1961.—(8.1×18×32.2.5).

Development of the lunar and solar perturbations in the motion of an artificial satellite. P. Musen et al. *N.A.S.A. T.N. D*-494. *Jan.* 1961.—(8.2×22.2).

Radio transmission through the plasma sheath around a lifting reentry vehicle. M. C. Ellis and P. W. Huber. N.A.S.A. T.N. D-507. *January* 1961.
Computations of plasma conditions about typical re-entry shapes are made to show effects of body bluntness, angle of attack, velocity, and altitude on the plasma frequency and collision frequency in the shock layer.—(8.2×32.2.5×1.4.4).

On the accuracy of an elliptical orbit determination. R. F. Weirauch. N.A.S.A. T.N. D-573. *Jan.* 1961.—(8.2×22.2).

The ion-trap results in "Exploration of the upper atmosphere with the help of the third Soviet Sputnik." E. C. Whipple. N.A.S.A. T.N. D-665. *Jan.* 1961.—(8.1×32.2.5).

Summary of rocket and satellite observations related to the ionosphere. J. C. Seddon. N.A.S.A. T.N. D-667. *Jan.* 1961.
Measurements of electron densities up to and above the F_2-maximum, columnar electron densities, electron density gradients, sporadic-*E*, and spread-*F* are discussed. An extensive bibliography is given.—(8.1×32.2.5).

Osculating elements derived from the modified Hansen's theory for the motion of an artificial satellite. A. Bailie and R. Bryant. N.A.S.A. T.N. D-568. *Jan.* 1961.—(8.2×22.2).

On the motion of a satellite in an asymmetrical gravitational field. P. Musen. N.A.S.A. T.N. D-569. *Jan.* 1961.—(8.2×22.2).

Approximate analysis of atmospheric entry corridors and angles. R. W. Luidens. N.A.S.A. T.N. D-590. *Jan.* 1961.
A simple closed-form approximate solution is developed as a function of maximum *g* load, initial entry velocity, and configuration lift-drag ratio, for vehicles operating at constant angle of attack and modulated angle of attack. The vehicle design and mode of operation that result in the deepest corridors are determined, and the effects of hot-gas radiation and a limiting Reynolds number on corridor depth are discussed.—(8.2×25.1).

FLIGHT TESTING

See also AERODYNAMICS—STABILITY AND CONTROL
AEROELASTICITY
AIRCRAFT OPERATION

A longitudinal control feel system for in-flight research on response feel. S. Faber and H. L. Crane. N.A.S.A. T.N. D-632. *Jan.* 1961.
Variable feel was provided from five sources: stick position, stick rate, normal acceleration, pitching acceleration, and pitching velocity.—(13.2×1.8.2.1).

Launch characteristics of the X-15 *research airplane as determined in flight. G. J. Matranga. N.A.S.A. T.N. D*-723. *Feb.* 1961.—(13.3×1.7.2×13.2×1.6.3).

HYDRODYNAMICS

The drag coefficients of parabolic bodies of revolution operating at zero cavitation number and zero angle of yaw. V. E. Johnson and T. A. Rasnick. N.A.S.A. T.R. R-86. 1961.—(17.1).

Théorie approchée de la houle pure et de la houle complexe. A. Daubert. Pubs. Sc. et Tech. 375. 1961. (*In French.*)—(17.1).

INSTRUMENTS AND EQUIPMENT

See EXTRA-ATMOSPHERIC TECHNOLOGY
INSTRUMENTS AND EQUIPMENT

MATHEMATICS

See also EXTRA-ATMOSPHERIC TECHNOLOGY

Application of describing-function analysis to the study of an on-off reaction control system. E. C. Lineberry and E. C. Foudriat. N.A.S.A. T.N. D-654. Jan. 1961.
The describing-function technique has been used in the analysis of an automatic reaction-control system for the upper stages of a missile during its burning and coasting phase. Limit-cycle characteristics, corresponding duty cycles, and system-parameter effects on these quantities were determined.—(22.2×27.3×25.1).

Investigation of third order contactor control systems with zeros in their transfer functions. I. Flügge-Lotz and T. Ishikawa. N.A.S.A. T.N. D-719. Jan. 1961.—(22×1.8.0.1×26).

METEOROLOGY

See also FATIGUE

Magnetic and ionospheric disturbances. Collected articles pertaining to sections III and V of the I.G.Y. program (Geomagnetism and earth currents, ionosphere). Yu. D. Kalinin. N.A.S.A. T.T. F-49. Jan. 1961.
Twelve reports by Soviet scientists originally read at a symposium on geomagnetic and ionospheric disturbances held during the Fifth Assembly of the Special Committee for the International Geophysical Year in July-Aug. 1958 in Moscow. Data obtained after the assembly have been added.—(24).

Investigation of the Cape Canaveral, Florida wind magnitude and wind shear characteristics in the ten to fourteen kilometer altitude region. W. W. Vaughan. N.A.S.A. T.N. D-556. Jan. 1961.

Temporal changes of temperature as a function of altitude for Patrick Air Force Base, Florida. J. W. Smith. N.A.S.A. T.N. D-562. January 1961—(24).

MISSILES

See also AERODYNAMICS—LOADS
STABILITY AND CONTROL
EXTRA-ATMOSPHERIC TECHNOLOGY
MATHEMATICS
POWER PLANTS

Étude comparative de quelques dispositifs de trajectographie utilisant des ondes entretenues non modulées: extension de la méthode au relevé de trajectoires de grande envergure. J. Zakheim. O.N.E.R.A. N.T. 63. 1961. (25.1×26).

NAVIGATION

See MATHEMATICS
MISSILES

POWER PLANTS

See also AERODYNAMICS—COMPRESSIBLE FLOW
STABILITY AND CONTROL
MATHEMATICS
SCIENCE—GENERAL
THERMODYNAMICS

Performance charts for multistage rocket boosters. J. S. MacKay and R. J. Weber. N.A.S.A. T.N. D-582. Jan. 1961.
Various stage combinations of RP—liquid-oxygen and hydrogen-liquid-oxygen propellants are assumed, but the results may be modified for other specific-impulse combinations with little error. The data are applicable to both orbital and escape missions. Examples illustrate how the charts may be used to determine optimum staging and thrust-weight ratios.—(27.3×8.2×25.3).

An ion rocket with an electron-bombardment ion source. H. R. Kaufman. N.A.S.A. T.N. D-585. Jan. 1961.—(27.7).

Large-scale wind-tunnel tests of exhaust ingestion due to thrust reversal on a four-engine jet transport during ground roll. W. H. Tolhurst et al. *N.A.S.A. T.N. D-686. January* 1961.
The data includes the free-stream dynamic pressure at the occurrence of exhaust gas ingestion in the outboard engine and the increment of drag due to thrust reversal for various modifications to both cascade-type and target-type thru reversers.—(27.1.1.1×5).

Internal-performance evaluation of a two-position divergent shroud ejector. J. R. Mihaloew and A. J. Stofan. N.A.S.A. T.N. D-762. Jan. 1961.—(27.1.1.1).

Internal-performance evaluation of two fixed-divergent-shroud ejectors. J. R. Mihaloew. N.A.S.A. T.N. D-763. Jan. 1961. —(27.1.1.1).

Die Messung schnellveränderlicher Drücke in Verbrennungskraftmaschinen. Teil 1. *Untersuchung über das Verhalten verschiedener Verstärker-und Elektrometerstufen. Ko Terada. D.F.L. Bericht* 133. 1960. (*In German.*)—(27.2.2).

FATIGUE

Atmospheric turbulence encountered by a Britannia aircraft on Polar and North Pacific routes. R. T. Sewell. N.R.C. Report L.R.-301. March 1961.
The results should be considered as of an interim nature only. Observations have been made, but no definite conclusions drawn as data available are not sufficient to provide a true statistical representation of conditions.—(31.2.4.1×31.3.1×24×3.6).

SCIENCE—GENERAL

See also AERODYNAMICS—COMPRESSIBLE FLOW
AIRCRAFT OPERATION
EXTRA-ATMOSPHERIC TECHNOLOGY

An investigation of the effect of high temperature on the Schumann-Runge ultraviolet absorption continuum of oxygen. J. S. Evans and C. J. Schexnayder. N.A.S.A. T.R. R-92. 1961. —(32.2.2×34.1).

Experimental study of a single-coil induced-electromotive-force plasma accelerator. C. W. Matthews and W. F. Cuddihy. N.A.S.A. T.N. D-639. Jan. 1961.—(32.2.1×27.7).

Wave theory of the Mach-Zehnder interferometer. H. K. Zienkiewicz. R. & M. 3173. 1961.—(32.2.4×1.12.1).

STRUCTURES

LOADS *see also* AERODYNAMICS—LOADS
AEROELASTICITY

An evaluation of effects of flexibility on wing strains in rough air for a large swept-wing airplane by means of experimentally determined frequency-response functions with an assessment of random-process techniques employed. T. L. Coleman et al. *N.A.S.A. T.R. R-70.* 1960.—(33.1.1×1.6.3).

Flight investigation of some effects of a vane-controlled gust-alleviation system on the wing and tail loads of a transport airplane. R. L. Schott and H. A. Hamer. N.A.S.A. T.N. D-643. Jan. 1961.
Different gust-alleviation configurations were investigated and the effects of each system on the normal acceleration and loads of the aeroplane were determined.—(33.1.1×1.6.3×5.1.5).

The flight measurement and analysis of tail loads due to pilot and aircraft response in symmetric manoeuvres. E. J. Beczy. N.R.C. Report L.R.-296. Jan. 1961.
Plots are given of the elevator angles and rates, of the angular accelerations, of the normal accelerations and the times to reach peak normal acceleration, and of the incremental positive and negative tail loads.—(33.1.1×1.6.2).

THERMODYNAMICS

See also SCIENCE—GENERAL

Combined effect of contraction ratio and chamber pressure on the performance of a gaseous-hydrogen-liquid-oxygen combustor for a given propellant weight flow and oxidant-fuel ratio. M. Hersch. N.A.S.A. T.N. D-129. Feb. 1961.—(34.1.1×27.3).

Constant entropy properties for an approximate model of equilibrium air. C. F. Hansen and M. E. Hodge. N.A.S.A. T.N. D-352. Jan. 1961.—(34.1).

Der Einfluss der Luftbewegung auf die Kraftstoffverteilung im Brennraum eines Dieselmotors mit Luftdrehung. F. Eisfeld. D.F.L.-Bericht 136. 1961. (*In German.*)—(34.1.1×27.2.1).